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BY WILLIAM NICHOLSON.

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THE Authors of Original Papers in the present Volume, are Mr. Frederick Accum; Mr. Irwine; Sir. A. N. Edelcrantz; Dr. T. C. Hope, F. R. S. Edinburgh; Anthony Carlisle, Esq.; M. B. Donkin; Mr. Ezekiel Walker; Thomas Thompson, M. D.; Dr. Boſtock; Mr. Dalton; Edward Howard, Esq. F. R. S.; Mr. A. Woolf; Andrew Duncan, M. D. F. R. S. Edinburgh; Dr. Prince; Mr. J. C. Hornblower, and Mr. G. Smart. Of Foreign Works, M. Regnier; Wiegleb; Guyton; Curaudau; Baunach; Payſſé; Parmentier; Steinacher; Schaub; Nicolai; Bouillon la Grange; Klaproth; Lomet; Tromſdorff; Seguin; Ritter; Thenard; Vauquelin; La Place; Guyton-Morveau; Von Hombolt; Raymond; Berthollet, and Haſſenfratz. And of English Memoirs abridged or extracted, Wm. Herſchell, L. L. D. F. R. S.; Richard Chenevix, Esq. F. R. S. and M. R. I. A.; Humphry Davy, Esq.; Wm. Hyde Wollaston, M. D. F. R. S.; Sir H. C. Englefield, Bart. F. R. S.; James Smithson, Esq. F. R. S. P. R. I.; Mr. James Woart; William Fairman, Esq.; Charles Hatchett, Esq. F. R. S.; The Right Hon. C. Greville, F. R. S.; Mr. John Dalton; Everard Home, Esq. F. R. S.; and Andrew Duncan, Junr. M. D. F. R. S. Edinburgh.

Of the Engravings the Subjects are, 1. Dr. Young's Apparatus for illustrating the Doctrine of Preponderance. 2. Apparatus for Experiments with Spouting-Fluids. 3. A Lock of Combination by M. Regnier. 4. Solar Phenomenon obſerved, by Sir H. Englefield, Bart. 5. Figure to ſhew the Proportion of the magnified Images of the ſame Star at different Times, by Dr. Herſchell. 6. Sir A. N. Edelcrantz's Method of Raiſing Water in Worm-Tubs, Condensers, &c. 7. Eudiometric Apparatus, by Dr. Hope. 8. Veſſel for Inclosing

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SEPTEMBER, 1803.

ARTICLE I.

Experiments and Obſervations on the Compound of Sulphur and Phoſphorus, and the dangerous Exploſions it makes when expoſed to Heat. By FREDERICK ACCUM, *Practical Chemiſt and Teacher of Chemiſtry. Communicated by the Author.*

MARGRAFF, I believe, was the firſt who noticed the combination of phoſphorus and ſulphur; and Pelletier examined afterwards this compound, and pointed out ſome of its properties *. The latter philoſopher ſhowed, at leaſt, that the compound reſulting from the union of phoſphorus and ſulphur, in different proportions, is infinitely more fuſible than either of them taken ſeparately. Repeating the experiments of the French philoſopher, I had no apprehenſion that the combination of theſe two ſimple bodies was attended, under certain circumſtances, with conſequences which might prove fatal to the chemical operator. And it is with the view of preventing my brother chemiſts from falling a ſacrifice to unexpected dangers, that I ſhall relate an accident, which might have been attended with the moſt dangerous conſequences, before I ſtate the properties which characteriſe the compound which is the ſubject of theſe lines. The accident

Compound of phoſphorus and ſulphur; when firſt examined.

not ſuppoſed to be attended with danger.

* Journal de Phyſique, xxxv. 383.

Very dangerous explosion from the combination of half an ounce of phosphorus, ten ounces of water and one ounce of sulphur on a sand heat.

cident alluded to, happened in the following manner. Half an ounce of phosphorus, cut into pieces of the size of a pea, was introduced into a Florence flask, containing about ten ounces of water; one ounce of sulphur broken into fragments of about the same size was added, and the whole placed on a heated sand-bath. In a few minutes the union of the phosphorus and sulphur was effected. On leaving the whole in the heated sand, for about ten minutes longer, the empty part of the flask became filled with dense white fumes, which increased more and more; being unable to observe what change was taking place, I carefully removed the flask out of the sand-bath, and agitated the fluid in such a manner, that the fused compound of phosphorus and sulphur still remained under the surface of the water. But the instant this was done, the whole exploded in my hand with a tremenduous report; the mixture of the burning phosphorus was thrown into my face, and occasioned very painful wounds; the pieces of the Florence flask were scattered all over the laboratory, as fine as sand, and the larger parts of the neck of this vessel were driven into my right hand, as well as into the wall, to a considerable depth.

Repetition of the experiment with smaller and also with larger quantities of the materials. Explosion as before.

Anxious to understand the nature of this unexpected explosion I again exposed to heat, in a similar manner, two drachms of phosphorus and half an ounce of coarsely powdered sulphur, in a small flask containing four ounces of water. The mixture, after having been left in a heated sand-bath for about ten minutes, exploded with prodigious violence, and a flash of fire rose up to the ceiling. The same experiment was repeated with larger quantities of phosphorus and sulphur, three successive times, with similar effects. These experiments were made at the laboratory, and in the presence of the Right Honourable Lord Camelford, who liberally supplied the materials for these and the following experiments, and permitted them to be made on his premises.

Another accidental explosion of the same nature.

Before I advance any thing further concerning the accension of this compound, I beg leave to relate one instance more of a similar nature, which happened lately in my own laboratory. Mr. Garden, a philosophical gentleman immersed into a vessel filled with warm water, a vial containing six ounces of phosphorus, to which had been previously added one drachm of a mixture of phosphorus and sulphur. The contents of the

the vial being liquified, (which was the intent of immerſing it into heated water) he removed the vial out of the fluid, taking care to cloſe its orifice with his finger, and then agitated it gently. The moment this was began the vial burſt to pieces with a report like a gun, the burning mixture was thrown in all directions, and the whole laboratory was filled for ſome hours with a very denſe cloud of white vapours.

Careful examination of the phenomena.

Being thus ſufficiently convinced of the danger which attends the combination of phoſphorus with ſulphur, under ſuch circumſtances, I introduced into a Wedgwood's tube cloſed at one end, two drachms of phoſphorus, and double that quantity of ſulphur. I then added four ounces of water, and cloſed the other extremity of the tube with a cork, into which a bended tube was cemented, which terminated under a glaſs cylinder filled with mercury, ſtanding inverted in a baſon containing the ſame fluid. I then reclined the tube, and applyed heat to that part which contained the phoſphorus and ſulphur; on increaſing the heat gradually a quantity of gas was collected, which amounted to nearly two quarts. But no exploſion took place.

Phoſphorus, ſulphur and water expoſed to diſtillation.

afforded a gas without exploſion.

To learn the nature of this gas, I transferred a quantity of it into the water apparatus, and agitated it in contact with that fluid ſtrongly for a few minutes. Its volume was now conſiderably diminiſhed. On repeating the experiment in diſtilled water, it was found that this fluid abſorbed nearly $\frac{1}{4}$ of its own bulk. On ſending up one part of atmoſpheric air into a cylinder holding ſix parts of this gas, an inſtantaneous inflammation enſued, the cylinder became filled with white fumes, and a white cruſt lined the inner ſurface of the glaſs. Finding thus that the gaſeous product was decompoſable by atmoſpheric air, I collected another quantity of gas, in a ſimilar manner as before; and mingled it gradually with oxigen gas till no further accenſion enſued. The gas left behind amounted to $\frac{1}{64}$ of the whole. It had all the properties of nitrogen gas.

which, added to common air, diminiſhed that fluid;

but took fire when a ſmall portion of common air was added to a larger of the gas.

The reſidue of this gas decompoſed by oxigen was azote.

The white flakes which were collected from the ſides of the glaſs cylinder, as well as from the ſurface of the mercury over which the experiments were made, attracted moiſture rapidly, and became converted into a cream-like fluid. They conſiſted of ſulphur, ſulphuric and phoſphoric acids.

The precipitates were ſulphur, ſulphuric and phoſphoric acids.

Oxigenized muriatic acid gas acted more violently than oxigen gas, when mingled with this gaseous compound over mercury; the result was a considerable detonation, accompanied with vivid green light and dense white vapours.

The gas was therefore a compound of hydrogen, sulphur, and phosphorus;

From the results of these experiments it becomes obvious, that the gas under examination was a compound of hydrogen, sulphur, and phosphorus. And if we reason from the nature of the production of this gas, it is evident, that, during the action of the phosphorus and sulphur upon water, the latter fluid is decomposed, though neither sulphur nor phosphorus singly taken, can effect this decomposition; this therefore is sufficient to account for the unexpected explosion before stated.

formed by the decomposition of water; though that fluid is not affected by either singly.

Phosphuret of sulphur can decompose water at the common temperature.

In order to see whether phosphuret of sulphur were capable of decomposing water in common temperatures, two ounces of it were covered in a vial with eight ounces of water, and put aside for further examination. The vial having been left unobserved, locked up in a closet, for some weeks; the cork was found to have been thrown out of the vial, and the whole inside of the closet, which had been painted with white lead, was completely blackened; the parts nearest to the orifice of the vial had a metallic aspect. The fluid which was decanted from the phosphuret of sulphur had a milky appearance, its odour was like that of water strongly impregnated with sulphurated hidrogen; its taste was uncommonly nauseous. It had a strong action on the greater number of metallic oxides.

On mingling it with concentrated nitrous acid, a considerable precipitate ensued, which after being dried on exposure to air, was luminous in the dark and became converted into sulphur. Another quantity of the water which was suffered to evaporate, spontaneously deposited crystals of a lemon yellow colour, but of an indeterminate figure. There remained, therefore, no doubt but that phosphuret of sulphur is capable of decomposing water at usual temperatures.

As phosphuret of sulphur decomposes air with greater rapidity than any other substance, it will be a good eudiometrical agent.

Phosphuret of sulphur, composed of three parts of phosphorus and one of sulphur, has also the property of decomposing atmospheric air with great rapidity. It may, therefore, be employed more advantageously for eudiometrical processes than either phosphorus, or the sulphurets of earths, alkalies, or metals. If into a *dry* glass tube closed at the top, and graduated

graduated into equi-diſtant parts, a quantity of phoſphuret of ſulphur, freed from adhering water, be poured, and agitated in the tube, ſo as to line a conſiderable part of it within, a vaſt quantity of white vapour is produced the moment the tube is immerſed in water; ſo as to exclude the air. The vapours will be abſorbed by the water, and when no further clouds appear, the proceſs is at an end. The reſiduary gas will then be found to be the quantity of nitrogen gas, which was contained in the air experimented upon in the tube. This proceſs is far more expeditious than the ſlow combuſtion of phoſphorus. I muſt, nevertheleſs, remark, that phoſphuret of ſulphur *like all other ſubſtances* hitherto employed for eudiometry, cannot be abſolutely depended upon for aſcertaining the abſolute quantity of oxigen, contained in a given portion of atmoſpheric air. For as ſoon as the abſorption of oxigen is compleated, the remaining nitrogen exerciſes an action upon the phoſphorus, by means of which its bulk becomes increaſed. From a number of experiments made with that view with this eudiometrical ſubſtance, I am led to believe that the volume of nitrogen gas, never increaſes ſo much as to $\frac{1}{3}$ part; conſequently the bulk of the reſiduum, diminiſhed by $\frac{1}{38}$ gives us the bulk of the nitrogen gas of the air examined; which bulk ſubtracted from the original maſs of air, indicates the proportion of oxigen gas contained in it.

This phoſphuret however, has the uſual imperfection of changing the reſidue.

The change is ſmall and may be allowed for.

Phoſphuret of ſulphur of the above compoſition alſo decompoſes nitrous acid with uncommon rapidity at common temperatures. If one part of phoſphuret of ſulphur be introduced in the cold into four or ſix of concentrated nitrous acid, a violent action takes place, the acid is decompoſed, and both the phoſphorus and ſulphur are oxigenized, at the expenſe of the oxigen of the nitric acid. A clear ſolution is obtained, from which phoſphoric and ſulphuric acid may be ſeparated in the uſual manner.

This phoſphuret decompoſes nitrous acid very rapidly.

Phoſphuret of ſulphur is ſoluble in expreſſed or fat oils. If one part of this compound, freed from adhering moiſture as much as poſſible, be triturated in a Wedgwood's mortar, with ſix parts of oil of almonds or olives, a liquid phoſphorus is obtained, which is far ſuperior to that produced in the uſual manner from mere phoſphorus. This liquid phoſphorus ſhines and gives a very fine liquid phoſphorus, with a beautiful yellow light. It may be rubbed over the face, hands, &c. without injury,

Phoſphuret of ſulphur is ſoluble in fat oils.

injury, provided the fluid be perfectly tranſparent, and conſequently contains not a particle of phoſphuret of ſulphur mechanically ſuſpended.

Which is uncommonly luminous.

The luminous property of this liquid phoſphorus is ſo conſiderable, that about 4oz. of it, when contained in a common ſize wine decanter, gives a ſufficient light to diſcern objects at a conſiderable diſtance in a large room, the moment the decanter is unſtopped.

Beautiful experiment of a luminous ſhower.

Equal parts of this liquid phoſphorus and oil of turpentine, when agitated together and poured out of any convenient perforated veſſel, exhibits a beautiful phenomenon, greatly reſembling a luminous rain, or ſhower of fire.

Caution againſt heat.

Though a liquid phoſphorus may be obtained, as directed before, no attempt ſhould be made to apply heat to a mixture of phoſphuret of ſulphur. For an exploſion was always the reſult whenever I attempted any proceſs of that kind.

Phoſphuret of ſulphur is ſoluble in ether.

Phoſphuret of ſulphur is ſoluble in ſulphuric and nitric ether. If either of theſe fluids be ſuffered to ſtand for ſome weeks, over a quantity of phoſphuret of ſulphur in a cloſed vial; part of the compound becomes diſſolved in the ether. If the ether be ſuffered to evaporate ſpontaneouſly, or when aſſiſted by heat, a multitude of exceedingly ſmall cryſtals are left behind, which ſhine in the dark with a brilliant yellow light. A piece of cloth dipped into this ether appears luminous in the dark all over, but in a few minutes this luminous appearance ceaſes, and the whole appears to be ſprinkled over with gems. If a feather, or piece of tow, be dipt in water, and then thrown into a bottle containing ether impregnated with phoſphuret of ſulphur; at the moment of contact of the two fluids, a ſudden light of a yellow colour ſpreads through the air, undulates along the ſurface of the fluid, and illuminates the whole bottle.

Luminous phenomena.

Phoſphuret of ſulphur in oil of turpentine, and other volatile oils.

Phoſphuret of ſulphur is ſoluble in pure or rectified oil of turpentine, oil of roſemary, oil of lavender, and in the reſt of the volatile oils met with in commerce. The ſolutions are all luminous in the dark, and depoſit the phoſphuret of ſulphur when ſlowly evaporated, in the form of needle-ſhaped cryſtals.

Sparingly in alcohol.

Highly rectified alcohol takes up a ſmall quantity of phoſphuret of ſulphur, the alcolic ſolution is decompoſable by the addition of water.

It takes fire in ox. mur. acid gas.

Phoſphuret of ſulphur takes fire ſpontaneouſly in oxigenized muriatic acid gas. If a ſmall quantity of dry phoſphuret of ſulphur

phur be introduced into a metallic ſpoon, and then immerſed in a bottle filled with oxigenized muriatic acid gas; the compound inſtantly kindles and burns with great vividneſs. The reſults of this experiment of courſe are phoſphoric, ſulphuric, and muriatic acids.

When already inflamed it continues to burn in nitrous gas and in the nitrous oxide.

Phoſphuret of ſulphur, when in a ſtate of inflammation burns alſo in nitrous gas, and in gaſeous oxide of nitrogen.

Combuſtion in the pneumatic vacuum is very advantageouſly performed on this compound.

If a piece of cotton be impregnated with phoſphuret of ſulphur, and then ſurrounded with wool or tow, and placed under the receiver of an air-pump, the compound ſhines with a beautiful yellow light, which increaſes in proportion as the air is more rarefied. On re-admitting a ſmall portion of air, a beautiful *Corona* or Aurora Borealis pervades the receiver. If a thermometer be included into the cotton containing the phoſphuret of ſulphur, it riſes in proportion as the light increaſes, and in this reſpect, as well as in the former, the phoſphuret of ſulphur anſwers better for this experiment, which was firſt noticed by Van Marum, who made uſe of phoſphorus.

No heat is developed when the combination of phoſphorus and ſulphur is made.

The formation of phoſphuret of ſulphur ſeems not to be attended with any change of temperature, as is ſaid to be always the caſe in all chemical combinations whatever. For if the phoſphorus and ſulphur be immerſed in heated water, at a diſtance from each other, together with a thermometer, no increaſe or decreaſe of temperature could be obſerved by means of the moſt delicate inſtrument.

It is more poiſonous than phoſphorus.

The compound of phoſphorus and ſulphur acts more violently in deſtroying animal life, than phoſphorus alone. A cat which had eaten two grains of phoſphorus repeatedly without impunity, died within half an hour after having ſwallowed one grain of phoſphuret of ſulphur.

Old Compton-Street, Soho,
Auguſt 16, 1803.

II. *Obſervations*

II.

(*Concluded from Page* 304. Vol. V)

Obſervations of the Tranſit of Mercury over the Diſk of the Sun; to which is added, an Inveſtigation of the Cauſes which often prevent the proper Action of Mirrors. *By* WILLIAM HERSCHEL, *LL. D. F. R. S.*

Mercury neatly defined.

WITH a 10-feet reflector, and magnifying power of 130, I ſaw the corrugations of the luminous ſolar ſurface, up to the very edge of the whole periphery of the diſk of Mercury.

10^h 27′. When the planet was ſufficiently advanced towards the largeſt opening of the northern zone, I compared the intenſity of the blackneſs of the two objects; and found the diſk of Mercury conſiderably darker, and of a more uniform black tint, than the area of the large opening.

10^h 32′. The preceding limb of Mercury cuts the luminous ſolar clouds with the moſt perfect ſharpneſs; whereas, in the great opening, the deſcending parapet, down the preceding ſide, was plainly viſible.

It ſhould be remarked, that the inſtrument here applied to the ſun, with the moderate power of 130, is the ſame 10-feet reflector which, in fine nights, when directed to very minute double ſtars, will ſhow them diſtinctly with a magnifier of 1000.

Great magnifying power not ſuitable to the ſun.

Having often attempted to uſe high magnifiers in viewing the ſun, I wiſhed to make another trial; though pretty well aſſured I ſhould not ſucceed, for reaſons which will appear hereafter.

With two ſmall double convex lenſes, both made of dark green glaſs, and one of them having the ſide which is neareſt the eye thinly ſmoked, in order to take off ſome light, I viewed the ſun. Their magnifying power was about 300; and I ſaw Mercury very well defined; but that complete diſtinctneſs, which enables us to judge with confidence of the condition of the object in view, was wanting.

With a ſingle eye-glaſs, ſmoked on the ſide towards the eye, and magnifying 460 times, I alſo ſaw Mercury pretty well defined; but here the ſun appeared ruddy, and no very minute objects could be perceived.

11^h

11^h 28′. The planet having advanced towards the preceding limb of the ſun, it was now time to attend to the appearances of the interior and exterior contacts. Obſervations of the contacts.

11^h 32′. 10-feet reflector. The whole diſk of Mercury is as ſharply defined as poſſible; there is not the leaſt appearance of any atmoſpheric ring, or different tinge of light, viſible about the planet. Not the leaſt ſign of any atmoſphere about Mercury.

11^h 37′. Appearances remain exactly as before.

11^h 42′. The ſharp termination of the whole mercurial diſk, appears to be even more ſtriking than before. This may be owing to its contraſt with the bright limb of the ſun, which, having many luminous ridges in the northern zone, is remarkably brilliant about the place of the planet.

11^h 44′. I was a few moments longer writing down the above than I ſhould have been, to ſee the interior contact ſo completely as I could have wiſhed; however, the thread of light on the ſun's limb was but juſt breaking, or broken; but no kind of diſtortion, either of the limb or of the diſk of Mercury, took place. At the interior contact, no diſtortion either of the ſun's limb or the diſk of Mercury,

The appearance of the planet, during the whole time of its emerging from the ſun, remained well defined, to the very laſt. nor during the emerging nor afterwards.

The following limb of Mercury remained ſharp till it reached the very edge of the ſun's diſk; and vaniſhed without occaſioning the ſmalleſt diſtortion of the ſun's limb, in going off, or ſuffering the leaſt alteration in its own figure.

As ſoon as the planet had quitted the ſun, the uſual appearance of its limb was ſo inſtantly and perfectly reſtored, that not the leaſt trace remained whereby the place of its diſappearance could have been diſtinguiſhed from any other adjacent part of the ſolar diſk.

It will not be amiſs to add, that very often, during the tranſit, I examined the appearance of Mercury with a view to its figure, but could not perceive the leaſt deviation from a ſpherical form; ſo that, unleſs its polar axis ſhould have happened to be ſituated, at the time of obſervation, in a line drawn from the eye to the ſun, the planet cannot be materially flattened at its poles. No ſigns of an oblate figure.

OBSERVATIONS AND EXPERIMENTS RELATING TO THE CAUSES WHICH OFTEN AFFECT MIRRORS, SO AS TO PREVENT THEIR SHOWING OBJECTS DISTINCTLY.

The action of reflecting teleſcopes is very different at different times.

It is well known to aſtronomers, that teleſcopes will act very differently at different times. The cauſe of the many diſappointments they may have met with in their obſervations, is however not ſo well underſtood.

Sometimes we have ſeen the failure aſcribed to certain tremors, as belonging to ſpecula; and remedies have been pointed out for preventing them. Not unfrequently again, the teleſcope itſelf has been condemned; or, if its goodneſs could not admit of a doubt, the weather in general has been declared bad, though poſſibly it might be as proper for diſtinct viſion as any we can expect in this changeable climate.

The experience acquired by many years of obſervation, will however, I believe, enable me now to aſſign the principal cauſe of the diſappointments to which we are ſo often expoſed. Unwilling to hazard any opinion that is not properly ſupported by facts, I ſhall have recourſe to a collection of occaſional obſervations. They have been made with ſpecula of undoubted goodneſs, ſo that every cauſe which impeded their proper action muſt be looked upon as extrinſic. I ſhall arrange theſe obſervations under different heads, that, when they have been related, there may remain no difficulty to draw a few general concluſions from them, which will be found to throw a conſiderable light upon our ſubject.

Moiſture in the Air.

Whether moiſture in the air impedes the action of teleſcopes.

(1.) October 5, 1781. I ſee double ſtars, with 460, completely well. The air is very damp.

(2.) Nov. 23, 1781. 15^h 30′. The morning is uncommonly favourable, and I ſee the treble ſtar ζ Cancri, with 460, in high perfection. The air is very moiſt, and intermixed with paſſing clouds.

(3.) Sept. 7, 1782. I viewed the double ſtar preceding 12 Camelopardalis,* with 932. In this, and ſeveral other fine nights which I have lately had, the condenſing moiſture on the tube of my teleſcope has been running down in ſtreams; which proves that damp air is no enemy to good viſion.

* See Phil. Tranſ. Vol. LXXV. Part I. page 68; II. 53.

(4.) Dec.

(4.) Dec. 28, 1782. 17^h 30′. The water condenſing on my tube keeps running down; yet I have ſeen very well all night. I was obliged to wipe the object-glaſs of my finder almoſt continually. The ſpecula, however, are not in the leaſt affected with the damp. The ground was ſo wet that, in the morning, ſeveral people believed there had been much rain in the night, and were ſurpriſed when I aſſured them there had not been a drop.

(5.) Feb. 19, 1783. I have ſeen perfectly well till now * that a froſt is coming on; though Datchet Common, which is juſt before my garden, is all under water; and the graſs on which I ſtand with my teleſcope is as wet as poſſible.

(6.) Feb. 26, 1783. All the ground is covered with ſnow; yet I ſee remarkable well.

(7.) March 8, 1783. The common before my garden is all under water; my teleſcope is running with condenſed vapour; not a breath of air ſtirring. I never ſaw better.

(8.) Auguſt 25, 1783. My teleſcope ran with water all the night. The ſmall ſpeculum, which ſometimes gathers moiſture, was never affected in the 7-feet tube, but was a little ſo in the 20-feet. The large eye-glaſſes and object-glaſſes of the finders, required wiping very often. I ſaw all night remarkably well.

Fogs.

Whether fogs impair the diſtinctneſs of teleſcopes.

(9.) Oct. 30, 1779, It grows very foggy, and the moon is ſurrounded with ſtrong nebuloſity; nevertheleſs, the ſtars are very diſtinct, and the teleſcope will bear a conſiderable power.

(10.) Auguſt 20, 1781. It is ſo foggy that I cannot ſee an object at the diſtance of 40 feet; yet the ſtars are very diſtinct in the teleſcope. By an increaſe of the fog, α Piſcium can no longer be ſeen by the eye; yet, in the teleſcope, it being double, I ſee both the ſtars with perfect diſtinctneſs.

(11.) Sept. 6, 1781. A fog is come on; yet I ſee very well.

(12.) Sept. 9, 1781. There is ſo ſtrong a fog, that hardly a ſtar leſs than 30° high is to be ſeen; and yet, in the teleſcope, at great elevations, I ſee extremely well.

* The time is not marked in the journal; but, from the number of the obſervations that had been made during the night, it muſt have been towards morning.

(13.) March

(13.) March 9, 1783. It is very foggy; yet in the telescope I see the stars without aberration, and they are very bright. α Serpentarii is without a single ray.

(14.) April 6, 1783. A very thick fog settles upon all my glasses; but the specula, even the 20-feet, which has so large a surface, remained untouched. I see perfectly well.

Frost.

Whether frost be an impediment to distinct vision by telescopes.

(15.) Nov. 15, 1780; five o'clock in the morning. An excellent speculum, No. 2, will not act properly; the frosty morning probably occasions an alteration in its figure. Another speculum, No. 1, acts but indifferently, though I have known it to shew very well formerly in a very hard frost: for instance, November 23, 1779, I saw with the same mirror, and a power of 460, the vacancy between the two stars of the double star Castor, without the least aberration.

(16.) Oct. 22, 1781. Frost seems to be no hindrance to perfect vision. The tube of my 7-feet telescope is covered with ice; yet I see very well.

(17.) Nov. 19, 1781. It freezes very hard, and the stars, even those which are 50° high, are very tremulous. I suspect their apparent diameters to be diminished; and, if I recollect right, this is not the first time that such a suspicion has occurred to me.

(18.) Jan. 10, 1782. My telescope would not act well, even at an altitude of 70 or 80 degrees. There is a strong frost.

(19.) Jan. 31, 1782. I cannot see with a power of 460, the stars seem to dance so unaccountably, and yet the air is perfectly calm: even at 60 or 70 degrees of altitude, vision is impaired.

(20.) Feb. 9, 1782. That frost is no hindrance to seeing well is evident; for, not only my breath freezes upon the side of the tube, but more than once have I found my feet fastened to the ground, when I have looked long at the same star.

(21.) Oct. 4, 1782. It froze very severely this night. At first, when the frost came on, I saw very badly, every object being tremulous; but, after some time, and at proper altitudes, I saw as well as ever. Between five and six o'clock in the morning, objects began to be tremulous again; occasioned, I suppose, by the coming on of a thaw.

(22.) Jan.

(22.) Jan. 1, 1783. I made a number of delicate obſervations this night, notwithſtanding, at four o'clock in the morning, my ink was frozen in the room; and, at about five o'clock, a 20-feet ſpeculum, in the tube, went off with a crack, and broke into two pieces. On looking at Fahrenheit's thermometer, I found it to ſtand at 11°.

(23.) May 6, 1783. It freezes, and in the teleſcope the ſtars ſeem to dance extremely.

Hoar-froſt.

(24.) Nov. 6, 1782. There is a thick hoar-froſt; yet I ſee extremely well. It ſeems to enlarge the diameters of the ſtars; but, as I ſee the minuteſt double ſtars well, the apparent enlargement of the diameters muſt be a deception. Hoar froſt; its effect on teleſcopes.

(25.) Dec. 22, 1782. There is a ſtrong hoar-froſt gathering upon the tubes of my teleſcopes; but I ſee very well.

Dry Air.

(26.) Dec. 21, 1782. The tube of my teleſcope is dry, and I do not ſee well. Dry air inimical to diſtinct viſion by teleſcopes.

(27.) April 30, 1783. The ſtars are extremely tremulous and confuſed; the outſide of the tube of my teleſcope is quite dry.

Northern Lights.

(28.) Sept. 25, 1781. There are very ſtrong northern lights; their flaſhing does not ſeem to interfere with teleſcopic viſion; but all objects appear tremulous, and indifferently defined. Northern lights do not ſeem to impede viſion by teleſcopes.

(29.) Aug. 30, 1782. There are very bright northern lights, in broad arches, with white ſtreaks; yet I ſee perfectly well.

(30.) March 26, 1783. An Aurora Borealis is ſo bright, that η Herculis, which it covers, can hardly be ſeen; yet, in the teleſcope, and with a power of 460, I find no difference. I compared the ſtar with γ Coronæ, which was in a bright part of the heavens, and in the teleſcope they appeared nearly alike. I ſuſpected η Herculis to be ſomewhat more tinged with red than it ſhould be; and examined it afterwards, when clear of the Aurora: it was indeed leſs red; but, as it had gained more altitude, the experiment was not deciſive.

Windy

Windy Weather.

Wind impairs the effect of teleſcopes.

(31.) Jan. 8, 1783. It is very windy. The diameters of the ſtars are ſtrangely increaſed, even thoſe at 60 and 70° of altitude. Every ſtar ſeems to be a little planet.

(32.) Jan. 9, 1783. Wind increaſes the apparent diameters of the ſtars.

(33.) Sept. 20, 1783. The night has been very windy; and I do not remember ever to have ſeen ſo ill, with ſuch a beautiful appearance of brilliant ſtar-light.

Fine in Appearance.

Weather apparently fine but unfavourable.

(34.) May 28, 1781. The evening, though fine in appearance, is not favourable. No inſtrument I have will act properly. The wind is in the eaſt.

(35.) Auguſt 30, 1781. The ſtars appear fine to the naked eye, ſo that I can ſee ε Lyræ very diſtinctly to be two ſtars; yet my teleſcope will ſhow nothing well. There are flying clouds, which, by their rapid motion, indicate a diſturbance in the upper regions of the air; though, excepting now and then a few guſts of wind, it is in general very calm. At a diſtance there are continual flaſhes of lightning, but I can hardly hear any thunder.

(36.) Sept. 14, 1781. I ſee very ſmall ſtars with the naked eye; but the teleſcope will not act ſo well as it ſhould.

(37.) Sept. 24, 1781. The evening is apparently fine; but, with the teleſcope, I can ſee neither η Coronæ nor μ Bootis double; nor indeed can I ſee any other ſtars well.

Over a Building.

Vicinity of a building renders the ſtars indiſtinct;

(38.) Auguſt 24, 1783. I viewed ε Bootis with 449,737, and 910, but ſaw it very indifferently. The ſtar was over a houſe.

(39.) Oct. 26, 1780. ε Bootis being near the roof of a houſe, I ſaw it not ſo diſtinctly as I could wiſh.

The Teleſcope lately brought out.

Recent expoſure of the teleſcope does not afford diſtinctneſs.

(40.) Oct. 10, 1780. 6^h 30′. Having but juſt brought out my teleſcope, it will not act well.

6^h 45′. The tube and ſpecula are now in order, and perform very well.

(41.) Jan. 11, 1782. To all appearance, the morning was very fine, but ſtill the teleſcope, when firſt brought out, would not act well. After half an hour's expoſure, it performed better.

Confined Place.

(42.) July 19, 1781. 13^h 15′. My teleſcope would not act well; and, ſuppoſing the exhalations from the graſs in my garden to affect viſion, I carried the teleſcope into the ſtreet, (the obſervation was made at Bath,) and found it to perform to admiration. A confined place prevents teleſcopes from acting well.

(43.) July 19, 1781. My teleſcope acted very well; but a ſlight field-breeze ſpringing up, and bruſhing through the ſtreet where my inſtrument was placed, it would no longer bear a magnifying power of 460.

Hazineſs and Clouds.

(44.) Sept. 22, 1783. The weather is now ſo hazy, that the double ſtar β Cygni is but barely viſible to the naked eye. This has taken off the rays of the large ſtar, ſo that I now ſee the ſmall one extremely well, which at other times it is ſo difficult to perceive, even with a magnifying power of 932. Remarkable effect of hazineſs and clouds.

(45.) Auguſt 13, 1781. A cloud coming on very gradually upon fixed ſtars, has this remarkable effect, that their apparent diameters diminiſh gradually to nothing.

(46.) July 7, 1780. The air was very hazy, but extremely calm. I had Arcturus in the field of view of the teleſcope, and, the hazineſs increaſing, it had a very beautiful effect on the apparent diameter of this ſtar, For, ſuppoſing the firſt of the points *, to repreſent the magnitude when brighteſt, I ſaw it gradually decreaſe, and aſſume, with equal diſtinctneſs, the form of all the ſucceeding points, from No. 1 to No. 10, in the order of the numbers placed over them. The laſt magnitude I ſaw it under, could certainly not exceed two-tenths of a ſecond; but was perhaps leſs than one. This leads to the diſcovery of one of the cauſes of the apparent magnitude of the fixt ſtars. One cauſe of the apparent diameters of the fixed ſtars.

* Theſe points will be inſerted in one of the plates in our next number. N.

Focal

Focal Length.

Obſervations in which the focal length and figure of the ſpeculum was altered by ſolar heat.

(47.) Nov. 14, 1801. The focal length of my 10-feet mirror increaſes by the heat of the ſun. I have often obſerved this before; the difference, by ſeveral trials, amounts to 8 hundredths of an inch.

(48.) Dec. 13, 1801. The focal length of my 10-feet mirror, while I was looking at the ſun, became ſhorter, contrary to what it uſed to do; but, there being a ſtrong froſt, I gueſs that the object metal grows colder, notwithſtanding its expoſure to the ſun's rays.

(49.) Nov. 9, 1802. 10^h 50′. The focus of my 7-feet glaſs mirror became 18 hundredths of an inch ſhorter, on being expoſed for about a minute to the ſun. The figure of the ſpeculum was alſo diſtorted; the foci of the inſide and outſide rays differing conſiderably, though its curvature, by obſervations on the ſtars, has been aſcertained to be ſtrictly parabolical.

12^h 0′. The ſame mirror, expoſed one minute to the action of the ſun, became 21 hundredths ſhorter in focal length.

The focus of a 10-feet metalline mirror, when expoſed one minute to the ſun's rays, became 15 hundredths of an inch longer than it was before.

(50.) January 9, 1803. When I looked with the glaſs 7-feet mirror, ſeveral times, a minute or two at the ſun, it ſhortened generally ,24, ,26, and ,30 of an inch, in focal length.

The obſervations which are now before us, appear to be ſufficient to eſtabliſh the following principle; namely,

General principle. *Uniform temperatures and moiſture in the air are requiſite to diſtinct viſion.*

"That in order to ſee well with teleſcopes, it is required that the temperature of the atmoſphere and mirror ſhould be uniform, and the air fraught with moiſture."

This being admitted, we ſhall find no difficulty in accounting for every one of the foregoing obſervations.

This doctrine applied to,

ſudden changes of temperature;

If an uniform temperature be neceſſary, a froſt after mild weather, or a thaw after froſt, will derange the performance of our mirrors, till either the froſt or the mild weather are ſufficiently ſettled, that the temperature of the mirror may accommodate itſelf to that of the air. For, till ſuch an uniformity with the open air, in the temperature of the mirror, the tube, the eye-glaſſes, and I would almoſt add the obſerver, be obtained, we cannot expect to ſee well. See obſervation 15, 17, 18, 19, and 23.

But

But, when a froſt, though very ſevere, becomes ſettled, the mirror will ſoon accommodate itſelf to the temperature; and we ſhall find our teleſcopes to act well. See obſervation 16, 20, 21, 22, 24, and 25.

This explains, with equal facility, why no teleſcope juſt brought out of a warm room can act properly. See obſervation 40 and 41. or of expoſure;

Nor can we ever expect to make a delicate obſervation, with high magnifying powers, when looking through a door, window, or ſlit in the roof of an obſervatory; even a confined place, though in the open air, will be detrimental. See obſervation 42 and 43. obſervations from a confined place;

It equally ſhows, that windy weather in general, which muſt occaſion a mixture of airs of different temperatures, cannot be favourable to diſtinct viſion. See obſervation 31, 32, and 33. windy weather;

The ſame remark will apply to Auroræ Boreales, when they induce, as they often do, a conſiderable change in the temperature of the different regions of air. See obſervation 28. auroræ boreales;

But, ſhould they not be accompanied by ſuch a change, there ſeems to be no reaſon why they ſhould injure viſion. See obſervation 29 and 30.

The warm exhalations from the roof of a houſe in a cold night, muſt diſturb the uniformity of the temperature of a ſmall portion of air; ſo that ſtars which are over the houſe, and at no conſiderable diſtance, may be affected by it. See obſervation 38 and 39. the roof of an houſe;

Sometimes the weather appears to be fine, and yet our teleſcopes will not act well. This may be owing to dryneſs occaſioned by an eaſterly wind; or to a change of temperature, ariſing from an agitation of the upper regions of the atmoſphere. See obſervation 34 and 35. weather apparently fine;

Or, poſſibly, to both theſe cauſes combined together. See obſervation 36 and 37.

If moiſture in the atmoſphere be neceſſary, dry air cannot be proper for viſion. See obſervation 26 and 27.

And therefore, on the contrary, dampneſs, and hazineſs of the atmoſphere, muſt be favourable to diſtinct viſion. See obſervation 1, 2, 3, 4, 6, and 8. damps, haze;

Fogs alſo, which certainly denote abundance of moiſture, muſt be very favourable to diſtinct viſion. See obſervation 9, 10, 11, 12, 13, and 14. fogs, &c.

Nay, if the obſervatory ſhould be ſurrounded by water, we need be under no apprehenſion on that account. Perhaps, were we to erect a building for aſtronomical purpoſes only, we ought not to object to grounds which are occaſionally flooded; the neighbourhood of a river, a lake, or other generally called damp ſituations. See obſervation 5 and 7.

It is however poſſible, that fogs and hazineſs may increaſe to ſuch a degree as, at laſt, to take away, by their interpoſition, all the light which comes from celeſtial objects; in which caſe, they muſt of courſe put an end to obſervation; but they will nevertheleſs be accompanied with diſtinct viſion to the very laſt. See obſervation 44, 45, and 46.

We have now only the four laſt obſervations to account for. They relate to the change of the focal length of mirrors in ſolar obſervations, and its attendant derangement of the foci of the different parts of the reflecting ſurface; and, as ſimplicity is one of the marks of the truth of a principle, I believe we need not have recourſe to any other cauſe than the change of temperature produced by the action of the ſolar rays that occaſion heat; which will be quite ſufficient to explain all the phenomena. But, in order to ſhow this in its proper light, I ſhall relate the following experiments.

1ſt Experiment.

Experiments on the change of focal length in mirrors by heat.

I placed a glaſs mirror, of 7-feet focal length, in the tube belonging to the teleſcope; and, having laid it open at the back, I prepared a ſtand, on which the iron uſed in my experiments on the terreſtrial rays that occaſion heat (ſee Phil. Tranſ. for 1800, Plate XVI. Fig. 1.) might be placed, ſo as to heat the mirror from behind, while I kept a certain object in the field of view of the teleſcope. Having meaſured the focal length, and alſo examined the figure of the mirror, which was parabolical, the heated iron was applied ſo as to be about $2\frac{1}{4}$ inches from the back of the glaſs mirror. The conſequence of this was, that a total confuſion in all the foci took place, ſo that the letters on a printed card in view, which before had been extremely diſtinct, became inſtantly illegible. In 15 ſeconds, the focus of the mirror was ſhortened 2,3 inches; in half a minute, 3,47 inches; and, at the end of the minute, I found it no leſs than 4,59 inches ſhorter than it had been before the application of the hot iron.

On

On repeating the experiment, but placing the heated iron no more than $\frac{3}{4}$ of an inch from the back of the mirror, its focal length, in $1\frac{1}{2}$ minute, became 5,33 inches ſhorter. Experiments on the change of foca. length in mirrors by heat.

I tried alſo a more moderate heat; and, placing the iron at 3 inches from the back, the focus of the mirror ſhortened in one minute 2,83 inches.

A thermometer placed in contact with the reflecting ſurface of the mirror, could hardly be perceived to have riſen, during the time in which the hot iron produced the alteration of the focal length.

2d Experiment.

Every thing remaining as before, I ſuſpended a ſmall globe of heated iron in front of the mirror, at one inch and a half from its vertex; and, in two minutes, the focus was lengthened 5,3 inches. The figure of the mirror was alſo deranged; ſo that the letters on the card could not be diſtinguiſhed.

I made a ſecond trial, with the ſuſpended iron a little more heated, and brought it as near the ſurface of the mirror as I judged it to be ſafe; ſince a contact would probably have cracked the mirror. In conſequence of this arrangement, the focus lengthened, in one minute, 1,64 inch.

On removing the heated iron, the mirror returned, in one minute, to within ,18 inch of its former focal length; and, at the end of the ſecond minute ſeemed to be nearly reſtored. But the diſagreement of the foci of the different parts of the reflecting ſurface might be perceived for a long time afterwards, and cauſed an indiſtinctneſs of viſion, which plainly indicated that, under ſuch circumſtances, the magnifying power of the teleſcope, 225, was more than it ought to be, in order to ſee well.

3d Experiment.

I now changed the glaſs mirror for a metalline one; and, on placing the heater near the back of it, the focus of the ſpeculum, in 30 ſeconds, became ,77 inch ſhorter. But, continuing the obſervation, inſtead of ſhortening ſtill farther in the next 30 ſeconds, it became ,3 inch longer, ſo that, at the end of a minute, it was only ,47 ſhorter than before the approach of the hot iron.

4th Experiment.

Experiments on the change of focal length in mirrors by heat.

When the ſmall heated globe of the 2d experiment was ſuſpended in front of the mirror, the focus lengthened ,27 inch in one minute; nor would the lengthening increaſe by leaving the hot iron longer in its poſition. The foci in this, as well as in the 3d experiment, were ſo much injured that they could not be meaſured with any preciſion; and it was evident, that high magnifying powers ought not to be uſed with a mirror of which the temperature is undergoing a continual change.

I repeated the experiment with the iron nearly red hot; and found the focus lengthened 1,48 inch in 30 ſeconds. Five minutes after the removal of the iron, the regularity of the figure of the mirror was pretty well reſtored.

With a moderate heat, I had, in 30 ſeconds, a lengthening of the focus, of ,57 inch; and, in about $1\frac{1}{2}$ minute after the removal of the heated iron, diſtinct viſion was nearly reſtored.

Theſe four experiments ſhow, that a change in the temperature of mirrors, occaſioned by heat, is attended with an alteration of their focal length; and alſo prove, that the figure of the reflecting ſurface is conſiderably injured, during the time that ſuch a change takes place. We are conſequently authoriſed to believe, that the ſmall alteration in the focus of a mirror expoſed to the rays of the ſun, ariſes from the ſame cauſe. For, ſince a thermometer, when the ſun is ſhining upon it, will ſhow that its temperature is altered, the action of the ſolar rays upon a mirror muſt be attended with a ſimilar effect in its temperature. See obſervation 47, 48, 49, and 50.

The ſame experiments will now alſo explain why the obſervations of the ſun, related in our tranſit of Mercury, between 10^h 32′ and 11^h 28′, were not attended with ſucceſs; for we have ſeen that heat occaſions a derangement in the action of the reflecting ſurface; and it follows that, under ſuch circumſtances, high magnifying powers cannot be expected to ſhow objects very diſtinctly.

If it ſhould be remarked, that I have not explained why the focus of a glaſs mirror ſhould ſhorten by the ſame rays of the ſun which lengthen that of a metalline ſpeculum, I confeſs that this at preſent does not appear; and, as it is not material to our purpoſe, I might paſs it over in ſilence. We are however

pretty well aſſured, that the alterations of the focal length muſt be owing to a dilatation of the glaſs or metal of which mirrors are made, and muſt be greateſt where moſt heat is applied. Our experiments therefore cannot agree perfectly with ſolar obſervations; for, in the glaſs mirror, the application of partial heat in front, muſt undoubtedly have been much ſtronger about the middle of the mirror (though the centre of it was ſometimes guarded by a braſs plate equal to the ſize of the ſmall ſpeculum) than at the circumference. But when, on the contrary, a mirror is expoſed to the ſun, every part of the ſurface will receive an equal portion of heat.

Experiments on the change of focal length in mirrors by heat.

It may alſo be ſaid, that I have pointed out a defect in teleſcopes uſed for ſolar obſervations, without aſſigning a cure for it. It will however be allowed, that tracing an evil to its cauſe muſt be the firſt ſtep towards a remedy. Had the imperfection of the figure brought on by the heat of the ſolar rays been of a regular nature, an elliptical ſpeculum might have been uſed to counteract the aſſumed hyperbolical form; or *vice verſâ*.

And now, as, properly ſpeaking, the derangement of the figure of a mirror uſed in obſerving the ſun, is not ſo much cauſed by the heat of its rays as by their partial application to the reflecting ſurface only, which produces a greater dilatation in front than at the back, there may be a poſſibility of counteracting this effect, by a contrary application of heat againſt the back, or by an interception of it on the front. But this we leave to future experiments.

III.

Obſervations on the Chemical Nature of the Humours of the Eye. *By* Richard Chenevix, *Eſq. F. R. S. and M. R. I. A.**

THE functions of the eye, ſo far as they are phyſical, have been found ſubject to the common laws of optics. It cannot be expected that chemiſtry ſhould clear up ſuch obſcure points of phyſiology, as all the operations of viſion appear to be; but, ſome acquaintance with the intimate nature of the ſubſtances

* From the Philoſophical Tranſactions, 1803.

which

which produce the effects, cannot fail to be a useful appendage to a knowledge of the mechanical structure of the organ.

Humours of the eye little known chemically.

The chemical history of the humours of the eye, is not of much extent.

Aqueous humour.

The aqueous humour had been examined by Bertrandi; who said, that its specific gravity was 975, and therefore less than that of distilled water. Fourcroy, in his *Système des Connoissances chimiques*, tells us, that it has a saltish taste; that it evaporates without leaving a residuum; but that it contains some animal matter, with some alkaline phosphate and muriate. These contradictions only prove, that we have no accurate knowledge upon the subject.

Vitreous humour.

The vitreous humour is not better known. Wintringham has given its specific gravity (taking water at 10000) as equal to 10024; but I am not acquainted with any experiments to investigate its chemical nature.

Crystalline lens.

We are told by Chrouet, that the crystalline lens affords, by destructive distillation, fetid oil, carbonate of ammonia, and water, leaving some carbon in the retort. But, destructive distillation, although it has given us much knowledge as to animal matter in general, is too vague a method for investigating particular animal substances.

I shall now proceed to mention the experiments I have made upon all the humours. I shall first relate those which were made upon the eyes of sheep, (they being the most easily procured,) and shall afterwards speak of those of the human body, and other eyes. I think it right to observe, that all these eyes were as fresh as they could be obtained.

SHEEPS' EYES.

Aqueous Humour.

Of sheeps' eyes: The aqueous humour. Water, albumen, gelatine and muriate of soda.

The aqueous humour is a clear transparent liquid, of the specific gravity of 10090 *, at 60° of Fahrenheit. When fresh, it has very little smell, or taste.

It causes very little change in the vegetable reactive colours; and this little would not, I believe, be produced immediately after death. I imagine it to be owing to a generation of ammonia, some traces of which I discovered.

* All these specific gravities are mean proportionals of several experiments. The eyes of the same species of animal, do not differ much in the specific gravity of their humours.

When

When expoſed to the air, at a moderate temperature, it evaporates ſlowly, and becomes ſlightly putrid.

When made to boil, a coagulum is formed, but ſo ſmall as hardly to be perceptible. Evaporated to dryneſs, a reſiduum remains, weighing not more than 8 per cent. of the original liquor.

Tannin cauſes a precipitate in the freſh aqueous humour, both before and after it has been boiled, and conſequently ſhows the preſence of gelatine.

Nitrate of ſilver cauſes a precipitate, which is muriate of ſilver. No metallic ſalts, except thoſe of ſilver, alter the aqueous humour.

From theſe and other experiments it appears, that the aqueous humour is compoſed of water, albumen, gelatine, and a muriate, the baſis of which I found to be ſoda.

I have omitted ſpeaking of the action of the acids, of the alkalis, of alcohol, and of other re-agents, upon this humour. It is ſuch as may be expected in a ſolution of albumen, of gelatine, and of muriate of ſoda.

Cryſtalline Humour.

To follow the order of their ſituation, the next of the humours is the cryſtalline.

Cryſtalline humour. Much larger proportion of albumen and gelatine.

This differs very materially from the others.

Its ſpecific gravity is 11000.

When freſh, it is neither acid nor alkaline. It putrifies very rapidly. It is nearly all ſoluble in cold water, but is partly coagulated by heat. Tannin gives a very abundant precipitate; but I could not perceive any traces of muriatic acid, when I had obtained the cryſtalline quite free from the other humours. It is compoſed, therefore, of a ſmaller proportion of water than the others, but of a much larger proportion of albumen and gelatine.

Vitreous Humour.

I preſſed the vitreous humour through a rag, in order to free it from its capſules; and, in that ſtate, by all the experiments I could make upon it, I could not perceive any difference between it and the aqueous humour, either in its ſpecific gravity, (which I have found to be 10090, like that of the other), or in its chemical nature.

Vitreous humour. The ſame as the aqueous.

M. Fourcroy

No phoſphate in theſe humours.

M. Fourcroy mentions a phoſphate, as contained in theſe humours; but I could not perceive any precipitation by muriate or nitrate of lime; nor did the alkalis denote the preſence of any earth, notwithſtanding M. Fourcroy's aſſertion of that fact.

HUMAN EYE.

Human eye; not chemically different from other eyes.

I could not procure a ſufficient quantity of theſe, freſh enough to multiply my experiments upon them. However, by the aſſiſtance of Mr. Carpue, Surgeon to his Majeſty's Forces, I fully convinced myſelf, that the humours of the human eye, chemically conſidered, did not contain any thing different from the reſpective humours of the eyes I had examined. The aqueous and vitreous humours contained water, albumen, gelatine, and muriate of ſoda; and the cryſtalline humour contained only water, albumen, and gelatine. The ſpecific gravity of the aqueous and vitreous humours, I found to be 10053; while that of the cryſtalline was 10790.

EYES OF OXEN.

So likewiſe the eyes of oxen.

I found the eyes of oxen to contain the ſame ſubſtances as the reſpective humours of other eyes. The ſpecific gravity of the aqueous and vitreous humours is 10088; and that of the cryſtalline 10765.

Probable law; that the ſmaller the eye the more does the denſity of the cryſtalline differ from that of the other humours.

What is particularly worthy of notice is, that the difference which appears to exiſt between the ſpecific gravity of the aqueous or vitreous humour and that of the cryſtalline, is much greater in the human eye than in that of ſheep, and leſs in the eye of the ox. Hence it would appear, that the difference between the denſity of the aqueous and vitreous humour and that of the cryſtalline, is in the inverſe ratio of the diameter of the eye, taken from the cornea to the optic nerve. Should further experiments ſhow this to be a univerſal law in nature, it will not be poſſible to deny that it is in ſome degree deſigned for the purpoſe of promoting diſtinct viſion.

The cryſtalline is conſiderably more denſe in approaching the centre.

In taking the ſpecific gravity of the aqueous and vitreous humours, no particular precaution is neceſſary, except that they ought to be as freſh as poſſible. But the cryſtalline humour is not of an uniform denſity throughout; it is therefore eſſential, that attention be given to preſerve that humour entire for this operation. I found the weight of a very freſh cryſtalline of an

ox

ox to be 30 grains; and its ſpecific gravity was, as I before ſtated, 10765. I then pared away all the external part, in every direction, till there remained but ſix grains of the centre, and the ſpecific gravity of theſe ſix grains, I found to be 11940. From this it would ſeem, that the denſity increaſes gradually, from the circumference to the centre.

It is very ſubject to diſorders from its coagulable nature.

It is not ſurpriſing that the cryſtalline humour ſhould be ſubject to diſorders, it being wholly compoſed of animal matter of the moſt periſhable kind. Fourcroy ſays, that it is ſometimes found oſſeous in advanced age. Albumen is coagulated by many methods; and, if we ſuppoſe that the ſame changes can take place in the living eye as in the dead animal matter of the chemiſts, it will be eaſy to account for the formation of the cataract; a diſorder which cannot be cured but by the removal of the opaque lens. If a ſufficient number of obſervations were made reſpecting the frequency of the cataract in gouty habits, ſome important concluſions might be drawn, as to the influence of phoſphoric acid, in cauſing the diſorder, by the common effect of acids, in coagulating albumen.

IV.

A Letter from Mr. IRVINE *concerning the late Dr.* IRVINE, *of Glaſgow, his Doctrine, which aſcribes the Diſappearance of Heat, without Increaſe of Temperature, to a change of Capacity in Bodies, and that of Dr.* BLACK, *which ſuppoſes Caloric to become latent by Chemical Combination with Bodies; with particular Remarks on the Miſtakes of Dr.* THOMPSON, *in his Accounts of theſe Doctrines.*

To Mr. NICHOLSON.

SIR,

Account given in the Encycl. Britt. of the inveſtigation of the zero of heat.

IN the article Chemiſtry in the Supplement to the Encyclopædia Britannica, in moſt reſpects excellently written, I could not fail to be ſtruck with the account there given of the theory of heat, and the mode of inveſtigating the natural zero adopted by the late Dr. Irvine. Had Dr. Thompſon been indiſputably accurate in *his* opinions, had a mathematically cloſe argument left no door for the entrance of doubt, he would ſcarcely even then

then have been juſtified in the terms he has uſed. But here we have every thing ſettled, and we are informed with the air of an ancient ſophiſt, that it is examined and found inſufficient. I am induced to make an obſervation or two on this examination, becauſe I am informed that the mathematical air which reigns through this part of this work has actually impoſed on many. For this time, however, I believe it can be ſhewn that it is no more than an air, and that this ſubject is not yet finally ſettled.

Dr. Thompſon's ſtatement of Dr. Irvine's theory of heat. That caloric abſorbed by ice on its converſion into water (without increaſe of temperature) only *ſeems to become latent* and is really *employed in keeping up the temperature while the capacity is increaſed*; but that this does not explain the act of fuſion.

Argument of Dr. Thompſon. The capacity of water is to that of ice as 10 : 9. —Ice during fuſion abſorbs what would have raiſed water 140°—it would therefore have raiſed the ice at leaſt as much if it had remained ſolid—whence it is inferred that the fuſed ice or water ought to have been raiſed 9—10ths or 126°, if the difference of capacity had alone operated in the caſe:

Dr. Thompſon ſays, at page 269, Suppl. Ency. Brit. "Dr. Irvine, of Glaſgow, advanced a theory on this ſubject different from that of Dr. Black. The ſpecific caloric of water being greater than that of ice, it requires a greater quantity of caloric to raiſe it to a given temperature than it does to raiſe ice. The caloric therefore does not become latent, it only ſeems to do ſo from the greater ſpecific caloric of water. This theory was zealouſly adopted by Dr. Crawford. Dr. Black obſerved very juſtly, that it did not account for the production of fluidity at all. The ſpecific caloric of water is indeed greater than that of ice; but how is the ice converted into water? This is an objection which the advocates for Dr. Irvine's or Dr. Crawford's theory, (as it has been improperly called) will not eaſily anſwer. Let us examine whether this theory accounts for the apparent loſs of caloric. It follows from Mr. Kirwan's experiments, that the ſpecific caloric of water is to that of ice as 10 to 9. Dr. Black proved, that as much caloric entered the ice as would have raiſed it had it been water, 140°. Let us ſuppoſe that it would only have raiſed the ice 140; in that caſe the melted ice ought to have been of the temperature of 158°, for 10 : 9 : : 140 : 126, but it was only 32°. Therefore 126° of caloric have diſappeared, and cannot be accounted for by the change of ſpecific caloric. Nor can the accuracy of Dr. Black's experiment be ſuſpected; it has been repeated in every part of the world, and varied in every poſſible way. We cannot doubt, therefore, that caloric unites with ſubſtances, and cauſes them to become fluid, or that there is in fact a caloric of fluidity different from ſpecific caloric."

Now nobody doubts Dr. Black's experiment, and it is not neceſſary to our argument to have any doubt on that ſubject. Dr. Thompſon gives it as a fair ſtatement of Dr. Irvine's theory,

ory, that the 140° entering ice during its change of forms, ſhould be leſſened in the ratio of 9 : 10, and ſhould therefore be 126°, or in other words, if the heat entering the ice were only enough to raiſe the ice 140°, that the temperature of the water ſhould be 158°. But it is eaſy to ſee that on this ſuppoſition the water would contain two portions of heat, namely, its original quantity from the natural zero to 32°, expreſſed in degrees according to the capacity of ice, and the ſuperadded portion from 32 to 158, expreſſed in degrees according to that of water. The ice would no doubt according to this ſtatement, become water without abſorbing any heat, and the water would be merely heated as in every other caſe. But Dr. Irvine, and after him Dr. Crawford, and the writers of elementary treatiſes for the laſt fifteen years, and I believe I may venture to ſay every one of our philoſophers, except Dr. Thompſon himſelf, have ſtated this doctrine of capacities to be, that every one of the thermometrical degrees expreſſing the whole of the heat contained in a body, are to be taken in proportion to its capacity; and therefore that if the ice ſuddenly changed its capacity, it would abſorb not merely a rateable proportion of what heat might be preſented to it, but an abſolute quantity to make up for its new capacity. For the heat neceſſary to raiſe the temperature of ice each degree from natural zero, is to the heat neceſſary to raiſe water each degree from the ſame point as the capacity of the one body is to that of the other. Ice cannot then as it acquires its new form, ſhew any augmentation of temperature till the differences between the heats of each degree from the loweſt point be made up. This difference amounts to the 140° found by the experiment, and cannot raiſe the water even the fraction of a degree in temperature, becauſe it is barely ſufficient for the demands of its new capacity. Had the 140° been applied to water, Dr. Thompſon's aſſertion of the riſe of temperature would have been juſt, but it can have no reference whatever to heat applied during a change of capacity.

But it was not raiſed at all: and therefore the 126° of heat are conſidered as having beyond doubt *combined with the body* and fuſed it.

Reply. Dr. Thompſon in his ſtatement of the change of capacity, attends to the ſuperadded heat only, and totally overlooks the heat before exiſting in the body.

Inadmiſſible conſequence. Dr. Irvine and all writers but Dr. Thompſon have conſidered the *whole heat* in a body at a given temperature as the *meaſure of its capacity*.

Ice in fuſion *abſorbs all the heat* preſented to it until its enlarged capacity is ſatisfied, and cannot till then have any increaſe of temperature.

The ſame arguments are repeated in page 271, and the ſame miſtake reigns through the whole. Is it not obvious, that if you would inſtantaneouſly increaſe the capacity of any body, it would immediately become colder, and its temperature ſink as much lower as its new capacity was higher. In the ſame way, if a body has its capacity ſuddenly increaſed, and at the ſame

If the capacity of a body were ſuddenly increaſed its temperature would fall; or it would continue ſtationary if due heat were added;

ſame time a quantity of heat added to it to make the whole heat in proportion to its new capacity, ſurely that body would continue exactly at the ſame thermometrical point. Now melting ice is that body; the 140° are demanded by new capacity,—would have been more if its capacity had been greater, and leſs if leſs. On the very principles therefore in diſpute, ice on becoming water ought to receive a quantity of heat, and that quantity is not governed by the proportion of 9 to 10, but the whole heats are in that proportion, and the 140° is only their difference. For all calculations on the alteration of temperature to be produced by a given portion of heat on a body, from the knowledge of the relation of its capacity to another, and of the number of degrees that other is raiſed by the ſame quantity, continue juſt only while the capacities continue in the ſame proportion.

and this is the caſe with ice.

hence, &c. as before.

Surely it is poſſible to form a notion that heat (140°,) may enter into water as the very cauſe of fuſion, ſo as to alter its ſtate and change its capacity; that very heat making the quantum due to the new capacity. It is the fact, that ice at 32° cannot bear the ſmalleſt addition of heat in that ſtate, but immediately begins to be converted into water. Let a quantity much leſs than repreſented by 140°, enter a given portion of ice, ſay $\frac{1}{2}$ a degree. A ſmall portion of the ice becomes water; its capacity being increaſed as 9 : 10, and here the proceſs ſtops; the entire maſs of ice and water remaining at 32°. More heat would alter the ſtate and enlarge the capacity of more of the ice, without raiſing its temperature; that is to ſay, would fuſe it. How this is done, or in other words, what it is that happens among the particles may not be eaſy to explain or to imagine; but in my apprehenſion this hypothetical part of the diſcuſſion would be at leaſt as obſcure in the doctrine of latent or combined heat, as in that which aſcribes the diſappearance of heat during fuſions to the enlargement of capacity.

The cauſe of fluidity aſcribed to the ſame action of heat which enlarges the capacities of bodies.

As to the diſagreement of reſults in the hands of different philoſophers concerning the natural zero, it is to be obſerved, that it is one thing to determine whether bodies have different capacities for heat, and another to expreſs theſe by accurate numbers. There can be no doubt of the fact that bodies have different capacities, and Dr. Irvine's theorem may enuntiate itſelf generally, by ſaying, that as the capacity of the ſolid is

The experiments on heat are moſtly too inaccurate to give the ſame depreſſion for the natural zero.

to

to that of the fluid, ſo is the whole heat of the ſolid to that of the fluid. But it is a widely different thing to determine preciſely the capacities of airs, and various other ſubſtances, experiments on which are ſubject to great inaccuracy, and muſt be carefully repeated many times before a philoſopher ſhould pretend to draw final, and ſtill leſs elementary, concluſions. Many of the experiments by which contradictory reſults have been obtained were made, (as I am told, for I have not yet had an opportunity of examining myſelf) by the calorimeter, an inſtrument liable to great and deſerved objections. Lavoiſier, by mixing ſulphuric acid and water made the natural zero 5803: my father by a ſimilar proceſs ſomewhere I think between 8 and 900. What will Dr. Thompſon make of this? Surely he will not conclude the theorem falſe, but one expementer wrong; moſt likely both inaccurate.

Dr. Thompſon has reaſoned as if the capacities of ice were not changed during the experiment.

It is very ſtrange indeed that Dr. Thompſon ſhould have found it difficult to underſtand how theſe 140° enter ice during fuſion without raiſing its temperature. All he ſays amounts to this, that a given quantity of heat will have more effect in raiſing the temperature of ice than that of water. But the ice muſt continue ice, and the water water, and a change of capacity alters the whole reaſoning. Before he can tell whether 140 ſhould be 14 or not, he muſt tell me the whole heat of ice, and let me examine whether that be to itſelf plus, 140 as 9 : 10 or not. He is exactly in the ſame error with regard to ſteam.

Dr. Irvine's method of aſcertaining the capacities of ice and water was by aſcertaining how much fine ſand produced an equal change of temperature in each.

As to a mode of finding the capacities of ice and water, of which my father was the undoubted diſcoverer, as well as of the general fact that all bodies change their capacity and form together, one of Irvine's modes was this: he mixed fine river ſand waſhed, or fine pounded glaſs of a given temperature with each, ſo as to raiſe or reduce each an equal number of degrees. Then the capacities were as the quantities of glaſs added to produce the ſame effect.

Dr. Thompſon aſſerts that the capacities are not proved to be as the abſolute heats.

Inſtance of iron, ſaid to have a greater capacity and contain leſs heat than water:—but miſtakenly.

Dr. Thompſon ſays, that there is no proof that the capacities of bodies are as their abſolute heats. The capacity of iron, he continues, is greater than that of water or even that of azotic gas, yet it is improbable that iron contains more heat than theſe ſubſtances. Now where did Dr. Thompſon find that the capacity of iron was greater than that of water and azotic gas? not in his own table ſurely. There iron by weight has capacity 0.1264, water 1.000, azotic gas, 0.7036, or as Dr. Crawford

Crawford ſays, .7936. Even of equal bulks that of iron is leſs than that of water, as I ſee in the ſame table. I can only ſuppoſe that Dr. Thompſon has ſtumbled on the ſpecific gravities inſtead of ſpecific heats, and there he would have been right enough.

The ſame experiment that determines ſpecific heats is alſo the teſt of the capacities. The abſolute heats are *(by inference* extended thro' the whole range from zero) taken to be in the ſame ratio.

I will ſay only a few words farther on the queſtion, whether bodies contain caloric in proportion to their ſpecific heats or not. Now firſt of all when they continue of the ſame capacities. Suppoſe the capacity of a body to be to that of water as 10 to 5, *i. e.* double. The ſame quantity of heat that raiſes water two degrees, raiſes it one; 2° more raiſe the body one more, and ſo on as far as we can go upwards, and the reverſe downwards in the ſcale. But ſuppoſe another body whoſe capacity is to that of water as 20 to 5, *i. e.* quadruple. Each 4° of the heat in water raiſe this new body one degree upwards, and the reverſe downwards, as far as we know. Now is it not probable here that the whole heats are in proportion to the capacities thus determined, ſince like thermometrical portions of heat taken out of each and applied to water affect it in that ratio? The ſpecific heats of bodies are ſaid to be different, when the ſame quantity of heat raiſes one a different number of degrees from the other, and that regularly as far as we can examine. Therefore each degree in each contains a quantity of heat proportional to its capacity. But the whole heat is made up of degrees, therefore the whole heats are proportional if the capacities remain the ſame. Dr. Thompſon grants this to *be* abſolute fact to the extent of our experiments.

If the capacities ſhould vary while the temperature changes, the only conſequence would be, that heat would be given out or abſorbed till the common temperature were reſtored, and then the abſolute heats would be proportioned to the new capacities.

But if the capacity be ſuppoſed to vary, firſt let it diminiſh. Then the quantity of heat given out is the difference of the whole heats of the two different ſtates of the body; and the whole heat of it in each ſtate is proportional to its capacity, and the whole heat of its higheſt capacity is equal to that of its loweſt plus the heat given out. Therefore the change of capacity has made no alteration on the whole heat of the body computed from a higher point, but will turn out the ſame as if no change had taken place.

If the capacity be ſuppoſed to increaſe, a ſimilar reaſoning would ſhew that the heat ſtill may be computed in the ſame way. Such are a few arguments on the other ſide of the queſtion

tion from Dr. Thompſon, by no means all that might be brought, nor dare I venture to hope ſo ſtated as to be beyond the reach of cenſure.

Intended publication of the writings of the late Dr. Irvine.

Many more curious points riſe before the imagination on ſo intereſting a ſubject as heat. As I hope, however, ſoon to be able to lay before the public ſome of my father's writings, I may on that occaſion have an opportunity of expreſſing myſelf at greater length than I can intrude in your journal.

I am,
SIR,
Your obedient humble ſervant,
WILLIAM IRVINE.

Bedford-Street, Covent-Garden.

V.

An Account of ſome Experiments and Obſervations on the Conſtituent Parts of certain Aſtringent Vegetables; and on their Operation in Tanning. By HUMPHRY DAVY, Eſq. *Profeſſor of Chemiſtry in the Royal Inſtitution.*

(Concluded from Page 256. Vol. V.)

IV. EXPERIMENTS AND OBSERVATIONS ON THE ASTRINGENT INFUSIONS OF BARKS, AND OTHER VEGETABLE PRODUCTIONS.

Infuſions of barks in water by gentle heat.

THE barks that I examined were furniſhed me by my friend Samuel Purkis, Eſq. of Brentford; they had been collected in the proper ſeaſon, and preſerved with care.

In making the infuſions, I employed the barks in coarſe powder; and, to expedite the ſolution, a heat of from 100 to 120° Fahrenheit was applied.

Of oak, willow, and Spaniſh cheſnut,

The ſtrongeſt infuſions of the barks of the oak, of the Leiceſter willow, and of the Spaniſh cheſnut, were nearly of the ſame ſpecific gravity, 1.05. Their taſtes were alike, and ſtrongly aſtringent; they all reddened litmus-paper; the infuſion of the Spaniſh cheſnut bark producing the higheſt tint; and that of the Leiceſter willow bark the feebleſt tint.

were chemically examined.

Two hundred grains of each of the infuſions were ſubmitted to evaporation; and, in this proceſs, the infuſion of the oak bark

Chemical examination of various barks.

bark furniſhed 17 grains of ſolid matter; that of the Leiceſter willow about $16\frac{1}{2}$ grains; and that of the Spaniſh cheſnut nearly an equal quantity.

The tannin given by theſe ſolid matters was, in that from the oak bark infuſion, 14 grains; in that from the willow bark infuſion $14\frac{1}{2}$ grains; and in that from the Spaniſh cheſnut bark infuſion 13 grains.

The reſidual ſubſtances of the infuſions of the Spaniſh cheſnut bark, and of the oak bark, ſlightly reddened litmus-paper, and precipitated the ſolutions of tin of a fawn colour, and thoſe of iron black. The reſidual matter of the infuſion of the willow bark, did not perceptibly change the colour of litmus; but it precipitated the ſalts of iron of an olive colour, and rendered turbid the ſolution of nitrate of alumine.

The ſolid matters produced by the evaporation of the infuſions, gave, by incineration, only a very ſmall quantity of aſhes, which could not have been more than $\frac{1}{150}$ of their original weights. Theſe aſhes chiefly conſiſted of calcareous earth and alkali; and the quantity was greateſt from the infuſion of cheſnut bark.

The infuſions were acted on by the acids, and the pure alkalis, in a manner very ſimilar to the infuſion of galls. With the ſolutions of carbonated alkalis, they gave denſe fawn-coloured precipitates. They were copiouſly precipitated by the ſolutions of lime, of ſtrontia, and of barytes; and, by lime-water in exceſs, the infuſions of oak and of cheſnut bark ſeemed to be deprived of the whole of the vegetable matter they held in ſolution.

By being boiled for ſome time with alumine, lime, and magneſia, they became almoſt colourleſs, and loſt their power of acting upon gelatine and the ſalts of iron. After being heated with carbonate of lime and carbonate of magneſia, they were found deeper coloured than before; and, though they had loſt their power of acting on gelatine, they ſtill gave denſe olive-coloured precipitates with the ſalts of iron.

In all theſe caſes, the earths gained tints of brown, more or leſs intenſe.

When the compound of the aſtringent principles of the infuſion of oak bark with lime, procured by means of lime-water, was acted on by ſulphuric acid, a ſolution was obtained, which pre-

precipitated gelatine, and contained a portion of the vegetable principles, and a certain quantity of ſulphate of lime; a ſolid fawn-coloured matter was likewiſe formed, which appeared to be ſulphate of lime, united to a little tannin and extractive matter.*

Copious precipitations by albumen.

The ſolutions were copiouſly precipitated by ſolution of albumen.

The precipitates they gave with gelatine were ſimilar in their appearance; their colour, at firſt, was a light tinge of brown, but they became very dark by expoſure to the air. Their compoſition was very nearly ſimilar; and, judging from the experiments on the quantity of gelatine employed in forming them, the compound of tannin and gelatine from the ſtrongeſt infuſion of oak bark, ſeems to conſiſt, in the 100 parts, of 59 parts of gelatine and 41 of tannin; that from the infuſion of Leiceſter willow bark, of 57 parts of gelatine and 43 of tannin; and that from the infuſion of Spaniſh cheſnut bark, of 61 parts of gelatine and 39 of tannin.

Experiments of tanning ſkin with the infuſions of barks.

Two pieces of calf-ſkin, which weighed when dry 120 grains each, were tanned; one in the ſtrongeſt infuſion of Leiceſter willow bark, and the other in the ſtrongeſt infuſion of oak bark. The proceſs was completed, in both inſtances, in leſs than a fortnight; when the weight of the leather formed by the tannin of the Leiceſter willow bark was found equal to 161 grains; and that of the leather formed by the infuſion of oak bark was equal to 164 grains.

Spent ouze or infuſion.

When pieces of ſkin were ſuffered to remain in ſmall quantities of the infuſions of the oak bark, and of the Leiceſter willow bark, till they were exhauſted of their tanning principle, it was found, that though the reſidual liquors gave olive-coloured precipitates with the ſolutions of ſulphate of iron, yet they were ſcarcely rendered turbid by ſolutions of muriate of tin; and there is every reaſon to ſuppoſe, that a portion of their extractive matter had been taken up with the tannin by the ſkin.

* M. Merat Guillot propoſes a method of procuring pure tannin, (*Annales de Chimie*, Tome XLI. p. 325.) which conſiſts in precipitating a ſolution of tan by lime-water, and decompoſing it by nitric or muriatic acid. The ſolution of the ſolid matter obtained in this way in alcohol, he conſiders as a ſolution of pure tannin; but, from the experiments above-mentioned, it appears, that it muſt contain, beſides tannin, ſome of the extractive matter of the bark; and it *may* likewiſe contain ſaline matter.

I attempted, in different modes, to obtain uncombined gallic acid from the ſolid matter produced by the evaporation of the barks, but without ſucceſs. When portions of this ſolid matter were expoſed to the degree of heat that is required for the production of gallic acid from Aleppo galls, no cryſtals were formed; and the fluid that came over gave only a brown colour to the ſolution of ſalts of iron, and was found to contain much acetous acid and empyreumatic oil.

When pure water was made to act, in ſucceſſive portions, upon oak bark in coarſe powder, till all its ſoluble parts were taken up, the quantities of liquor laſt obtained, though they did not act much upon ſolution of gelatine, or perceptibly reddened litmus-paper, produced a denſe black with the ſolution of ſulphate of iron: by evaporation, they furniſhed a brown matter, of which a part was rendered inſoluble in water by the action of the atmoſphere; and the part ſoluble in water was not in any degree taken up by ſulphuric ether; ſo that, if it contained gallic acid, it was in a ſtate of intimate union with extractive matter.

Slow tanning appears to add leſs to the weight of the leather than quick, and more of the mucilage is taken up.

Two pieces of calf-ſkin, which weighed when dry 94 grains each, were ſlowly tanned; one by being expoſed to a weak infuſion of the Leiceſter willow bark, and the other by being acted upon by a weak infuſion of oak bark. The proceſs was completed in about three months; and it was found, that one piece of ſkin had gained in weight 14 grains, and the other piece about $16\frac{1}{2}$ grains. This increaſe is proportionally much leſs than that which took place in the experiment on the proceſs of quick tanning. The colour of the pieces of leather was deeper than that of the pieces which had been quickly tanned; and, to judge from the properties of the reſidual liquors, more of the extractive matters of the barks had been combined with them.

The experiments of Mr. Biggin * have ſhown, that ſimilar barks, when taken from trees at different ſeaſons, differ as to the quantities of tannin they contain: and I have obſerved, that the proportions of the aſtringent principles in barks, vary conſiderably according as their age and ſize are different; beſides, theſe proportions are often influenced by accidental circumſtances, ſo that it is extremely difficult to aſcertain their diſtinct relations to each other.

* Phil. Tranſ. for 1799, page 299.

In

In every aſtringent bark, the interior white bark (that is, the part next to the alburnum) contains the largeſt quantity of tannin. The proportion of extractive matter is generally greateſt in the middle or coloured part: but the epidermis ſeldom furniſhes either tannin or extractive matter.

The interior coat of bark contains moſt tannin; the middle moſt extractive; and the epidermis little of either.

The white cortical layers are comparatively moſt abundant in young trees; and hence their barks contain, in the ſame weight, a larger proportion of tannin than the barks of old trees. In barks of the ſame kind, but of different ages, which have been cut at the ſame ſeaſon, the ſimilar parts contain always very nearly the ſame quantities of aſtringent principles; and the interior layers afford about equal portions of tannin.

Young trees afford more tannin.

An ounce of the white cortical layers of old oak bark, furniſhed, by lixiviation and ſubſequent evaporation, 108 grains of ſolid matter; and, of this, 72 grains were tannin. An equal quantity of the white cortical layers of young oak produced 111 grains of ſolid matter, of which 77 were precipitated by gelatine.

Relative quantities.

An ounce of the interior part of the bark of the Spaniſh cheſnut, gave 89 grains of ſolid matter, containing 63 grains of tannin.

The ſame quantity of the ſame part of the bark of the Leiceſter willow, produced 117 grains, of which 79 were tannin.

An ounce of the coloured or external cortical layers from the oak, produced 43 grains of ſolid matter, of which 19 were tannin.

From the Spaniſh cheſnut, 41 grains, of which 14 were tannin.

And, from the Leiceſter willow, 34 grains, of which 16 were tannin.

In attempting to aſcertain the relative quantities of tannin in the different *entire* barks, I ſelected thoſe ſpecimens which appeared ſimilar with regard to the proportions of the external and internal layers, and which were about the average thickneſs of the barks commonly uſed in tanning, namely, half an inch.

Of theſe barks, the oak produced, in the quantity of an ounce, 61 grains of matter diſſolved by water, of which 29 grains were tannin.

The Spaniſh cheſnut 53 grains, of which 21 were tannin.

And the Leiceſter willow 71 grains, of which 33 were tannin.

The proportions of theſe quantities, in reſpect to the tanning principle, are not very different from thoſe eſtimated in Mr. Biggin's table.*

Properties of the reſiduual portion of infuſed barks.

The reſidual ſubſtances obtained in the different experiments, differed conſiderably in their properties; but certain portions of them were, in all inſtances, rendered inſoluble during the proceſs of evaporation. The reſiduum of the cheſnut bark, as in the inſtance of the ſtrongeſt infuſion, poſſeſſed ſlightly acid properties; but more than $\frac{3}{4}$ of its weight conſiſted of extractive matter. All the reſiduums in ſolution, as in the other caſes, were precipitated by muriate of tin; and, after this precipitation, the clear fluids acted much more feebly than before on the ſalts of iron; ſo that there is great reaſon for believing, that the power of aſtringent infuſions to precipitate the ſalts of iron black, or dark coloured, depends partly upon the agency of the extractive matters they contain, as well as upon that of the tanning principle and gallic acid.

Elm and willow bark.

In purſuing the experiments upon the different aſtringent infuſions, I examined the infuſions of the bark of the elm and of the common willow. Theſe infuſions were acted on by reagents, in a manner exactly ſimilar to the infuſions of the other barks: they were precipitated by the acids, by ſolutions of the alkaline earths, and of the carbonated alkalis; and they formed, with the cauſtic alkalis, fluids not precipitable by gelatine.

An ounce of the bark of the elm, furniſhed 13 grains of tannin.

The ſame quantity of the bark of the common willow, gave 11 grains.

The reſidual matter of the bark of the elm, contained a conſiderable portion of mucilage; and that of the bark of the willow, a ſmall quantity of bitter principle.

Infuſions of ſumachs from Sicily and Malaga.

The ſtrongeſt infuſions of the ſumachs from Sicily and Malaga, agree with the infuſions of barks, in moſt of their properties; but they differ from all the other aſtringent infuſions that have been mentioned, in one reſpect; they give denſe precipitates with the cauſtic alkalis. Mr. Prouſt has ſhown, that ſumach contains abundance of ſulphate of lime; and it is probably to this ſubſtance that the peculiar effect is owing.

From an ounce of Sicilian ſumach, I obtained 165 grains of matter ſoluble in water, and, of this matter, 78 grains were tannin.

* Philoſophical Tranſactions for 1799, p. 263.

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An ounce of Malaga ſumach, produced 156 grains of ſoluble matter, of which 79 appeared to be tannin.

Of Myrobalans.

The infuſion of Myrobalans * from the Eaſt Indies, differed from the other aſtringent infuſions chiefly by this circumſtance, that it effervesced with the carbonated alkalis; and it gave with them a denſe precipitate, that was almoſt immediately rediſſolved. After the tannin had been precipitated from it by gelatine, it ſtrongly reddened litmus-paper, and gave a bright black with the ſolutions of iron. I expected to be able to procure gallic acid, by diſtillation from the Myrobalans; but in this I was miſtaken; they furniſhed only a pale yellow fluid, which gave merely a ſlight olive tinge to ſolution of ſulphate of iron.

Skin was ſpeedily tanned in the infuſion of the Myrobalans; and the appearance of the leather was ſimilar to the appearance of that from galls.

The ſtrongeſt infuſions of the teas are very ſimilar, in their agencies upon chemical teſts, to the infuſions of catechu.

An ounce of Souchong tea, produced 48 grains of tannin.

The ſame quantity of green tea, gave 41 grains.

Dr. Maton has obſerved, that very little tannin is found in cinchona, or in the other barks ſuppoſed to be poſſeſſed of febrifuge properties. My experiments tend to confirm the obſervation. None of the infuſions of the ſtrongly bitter vegetable ſubſtances that I have examined, give any precipitate to gelatine. And the infuſions of quaſſia, of gentian, of hops, and of chamomile, are ſcarcely affected by muriate of tin; ſo that they likewiſe contain very little extractive matter.

In all ſubſtances poſſeſſed of the aſtringent taſte, there is great reaſon to ſuſpect the preſence of tannin; it even exiſts in ſubſtances which contain ſugar and vegetable acids. I have found it in abundance in the juice of ſloes; and my friend Mr. Poole, of Stowey, has detected it in port wine.

V. GENERAL OBSERVATIONS.

Probability that there are different ſpecies of tanning matter.

Mr. Prouſt has ſuppoſed, in his paper upon tannin and its Species,† that there exiſt different ſpecies of the tanning principle, poſſeſſed of different properties, and different powers

* The Myrobalans uſed in theſe experiments are the fruit of the Terminalia Chebula. RETZ. *Obs. Botan.* Faſc. V. p. 31.

† *Annales de Chimie*, Tome XLI. p. 332.

of

of acting upon re-agents, but all precipitable by gelatine. This opinion is ſufficiently conformable to the facts generally known concerning the nature of the ſubſtances which are produced in organiſed matter; but it cannot be conſidered as proved, till the tannin in different vegetables has been examined in its pure or inſulated ſtate. In all the vegetable infuſions which have been ſubjected to experiment, it exiſts in a ſtate of union with other principles; and its properties muſt neceſſarily be modified by the peculiar circumſtances of its combination.

The ſpecific agencies of tannin in all infuſions are the ſame.

From the experiments that have been detailed it appears, that the *ſpecific* agencies of tannin in all the different aſtringent infuſions are the ſame. In every inſtance, it is capable of entering into union with the acids, alkalis, and earths; and of forming inſoluble compounds with gelatine, and with ſkin. The infuſions of the barks affect the greater number of re-agents in a manner ſimilar to the infuſion of galls; and, that this laſt fluid is rendered green by the carbonated alkalis, evidently depends upon the large proportion of gallic acid it contains. The infuſion of ſumach owes its characteriſtic property, of being precipitated by the cauſtic alkalis, to the preſence of ſulphate of lime; and, that the ſolutions of catechu do not copiouſly precipitate the carbonated alkalis, appears to depend upon their containing tannin in a peculiar ſtate of union with extractive matter, and uncombined with gallic acid or earthy ſalts.

Its affinities and habitudes.

In making ſome experiments upon the affinities of the tanning principle, I found that all the earths were capable of attracting it from the alkalis: and, ſo great is their tendency to combine with it, that, by means of them, the compound of tannin and gelatine may be decompoſed without much difficulty; for, after pure magneſia had been boiled for a few hours with this ſubſtance diffuſed through water, it became of a red-brown colour, and the fluid obtained by filtration produced a diſtinct precipitate with ſolution of galls. The acids have leſs affinity for tannin than for gelatine; and, in caſes where compounds of the acids and tannin are acted on by ſolution of gelatine, an equilibrium of affinity is eſtabliſhed, in conſequence of which, by far the greateſt quantity of tannin is carried down in the inſoluble combination. The different neutral ſalts have, comparatively, feeble powers of attraction for the tanning principle; but, that the precipitation they occaſion in aſtringent ſolutions,

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is not ſimply owing to the circumſtance of their uniting to a portion of the water which held the vegetable ſubſtances in ſolution, is evident from many facts, beſides thoſe which have been already ſtated. The ſolutions of alum, and of ſome other ſalts which are leſs ſoluble in water than tannin, produce, in many aſtringent infuſions, precipitates as copious as the more ſoluble ſaline matters; and ſulphate of lime, and other earthy neutral compounds, which are, comparatively ſpeaking, inſoluble in water, ſpeedily deprive them of their tanning principle.

It is not always exhibitable by gelatine.

From the different facts that have been ſtated, it is evident that tannin may exiſt in a ſtate of combination in different ſubſtances, in which its preſence cannot be made evident by means of ſolution of gelatine; and, in this caſe, to detect its exiſtence, it is neceſſary to have recourſe to the action of the diluted acids.

Skins in tanning abſorb extractive matter;

In conſidering the relations of the different facts that have been detailed, to the proceſſes of tanning and of leather-making, it will appear ſufficiently evident, that when ſkin is tanned in aſtringent infuſions that contain, as well as tannin, extractive matters, portions of theſe matters enter, with the tannin, into chemical combination with the ſkin. In no caſe is there any reaſon to believe that gallic acid is abſorbed in this proceſs; and M. Seguin's ingenious theory of the agency of this ſubſtance, in producing the deoxigenation of ſkin, ſeems ſupported by no proofs. Even in the formation of glue from ſkin, there is no evidence which ought to induce us to ſuppoſe that it loſes a portion of oxigen; and the effect appears to be owing merely to the ſeparation of the gelatine, from the ſmall quantity of albumen with which it was combined in the organiſed form, by the ſolvent powers of water.

and the leather has its quality affected by it;

The different qualities of leather made with the ſame kind of ſkin, ſeem to depend very much upon the different quantities of extractive matter it contains. The leather obtained by means of infuſion of galls, is generally found harder, and more liable to crack, than the leather obtained from the infuſions of barks; and, in all caſes, it contains a much larger proportion of tannin, and a ſmaller proportion of extractive matter.

Soft durable leather by ſlow tanning.

When ſkin is very ſlowly tanned in weak ſolutions of the barks, or of catechu, it combines with a conſiderable proportion of extractive matter; and, in theſe caſes, though the increaſe of

of weight of the ſkin is comparatively ſmall, yet it is rendered perfectly inſoluble in water; and is found ſoft, and at the ſame time ſtrong.

The ſaturated aſtringent infuſions of barks contain much leſs extractive matter, in proportion to their tannin, than the weak infuſions; and, when ſkin is quickly tanned in them, common experience ſhows that it produces leather leſs durable than the leather ſlowly formed.

The common opinion in favour of what is called feeding of the leather probably juſt.

Beſides, in the caſe of quick tanning by means of infuſions of barks, a quantity of vegetable extractive matter is loſt to the manufacturer, which might have been made to enter into the compoſition of his leather. Theſe obſervations ſhow, that there is ſome foundation for the vulgar opinion of workmen, concerning what is technically called the *feeding* of leather in the ſlow method of tanning; and, though the proceſſes of the art may in ſome caſes be protracted for an unneceſſary length of time, yet, in general, they appear to have arrived, in conſequence of repeated practical experiments, at a degree of perfection which cannot be very far extended by means of any elucidations of theory that have as yet been made known.

On the firſt view it appears ſingular that, in thoſe caſes of tanning where extractive matter forms a certain portion of the leather, the increaſe of weight is leſs than when the ſkin is combined with pure tannin; but the fact is eaſily accounted for, when we conſider that the attraction of ſkin for tannin muſt be probably weakened by its union with extractive matter; and, whether we ſuppoſe that the tannin and extractive matter enter together into combination with the matter of ſkin, or unite with ſeparate portions of it, ſtill, in either caſe, the primary attraction of tannin for ſkin muſt be, to a certain extent, diminiſhed

Vegetables are of value for tanning not merely by the matter that glue can precipitate.

In examining aſtringent vegetables in relation to their powers of tanning ſkin, it is neceſſary to take into account, not only the quantity they contain of the *ſubſtance* precipitable by gelatine, but likewiſe the quantity, and the nature, of the extractive matter; and, in caſes of compariſon, it is eſſential to employ infuſions of the ſame degree of concentration.

Catechu is the moſt powerful tanning material.

It is evident, from the experiments detailed in the IIId ſection, that of all the aſtringent ſubſtances which have been as yet examined, catechu is that which contains the largeſt proportion of tannin; and, in ſuppoſing, according to the common eſtimation, that from four to five pounds of common oak bark

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are required to produce one pound of leather, it appears, from the various ſynthetical experiments, that about half a pound of catechu would anſwer the ſame purpoſe *.

Alſo, allowing for the difference in the compoſition of the different kinds of leather, it appears, from the general detail of facts, that one pound of catechu, for the common uſes of the tanner, would be nearly equal in value to $2\frac{1}{4}$ pounds of galls, to $7\frac{1}{2}$ pounds of the bark of the Leiceſter willow, to 11 pounds of the bark of the Spaniſh cheſnut, to 18 pounds of the bark of the elm, to 21 pounds of the bark of the common willow, and to 3 pounds of ſumach. Its comparative value.

Various menſtruums have been propoſed for the purpoſe of expediting and improving the proceſs of tanning, and, amongſt them, lime-water and the ſolutions of pearl-aſh: but, as theſe two ſubſtances form compounds with tannin which are not decompoſable by gelatine, it follows that their effects muſt be highly pernicious; and there is very little reaſon to ſuppoſe, that any bodies will be found which, at the ſame time that they increaſe the ſolubility of tannin in water, will not likewiſe diminiſh its attraction for ſkin. Lime and other additions are probably hurtful.

VI.

An eaſy Method of raiſing Water for the Purpoſes of Refrigeration in Diſtilleries, Steam Condenſers, &c. *By* SIR A. N. EDELCRANTZ. *Communicated by the Inventor.*

THE method exhibited in the ſketch, Fig. II. Plate IV. being capable of ſaving near thirty feet of the height to which water may be required to be pumped, for the uſes mentioned in the title, appeared too ſimple and ingenious, when the learned inventor did me the honour to mention it in converſation, for me not to require permiſſion to communicate it to my readers. Introduction.

* This eſtimation agrees very well with the experiments lately made by Mr. Purkis, upon the tanning powers of Bombay catechu in the proceſſes of manufacture, and which he has permitted me to mention. Mr. Purkis found, by the reſults of different accurate experiments, that one pound of catechu was equivalent to ſeven or eight of oak bark.

The principle of the ſyphon applied to produce a current of water through a worm tub.

If the worm tube were open at the top as uſual, it is evident that all the water employed for cooling, would require to be raiſed by ſome mechanic force as high as the ſurface; ſuppoſe twenty feet. But as this water is not wanted for uſe at that elevation, but is only required to give out its heat, and then fall down again; it is clear that this fall may be applied to raiſe a conſiderable portion of what is to follow. Various means might be deviſed for ſuch an application; the ſimpleſt and moſt effectual, no doubt, is that to which Sir A. N. has given the preference, namely to convert the whole apparatus into a ſyphon.

Deſcription of the apparatus.

Suppoſe the worm tub to be cloſed at top; the cold water conveyed into it at the bottom from the veſſel A, and carried off heated at top by the pipe B into the overflowing veſſel C. Let us ſuppoſe the level in A to be two feet higher than that in C, and a current will be kept up through the whole fluid as long as may be deſired.

Whether the eſcape of gas would prevent or impede the effect.

It muſt occur to the experienced engineer that gas or air will eſcape from the water, eſpecially when heated and defended from the preſſure of the atmoſphere. But this may be obviated by attending to a few neceſſary circumſtances. Firſt, the elevation need not be very great, and leſs gas will eſcape; ſecondly, the temperature may be kept down by a large current through pipes of conſiderable diameter; and thirdly, it is practicable by various contrivances, that an interior float ſhall give notice when the gas has lowered the ſurface of the water beneath it to a certain point, and this may either warn an attendant to pump it out, or it may diſcharge an apparatus to produce the ſame effect without the immediate exertion of labour each individual time.

Concerning theſe and other ſecondary points, I ſhall not, however, enlarge; having enough to regret from the neceſſary imperfection of this deſcription, taken from the converſation of the inventor, inſtead of being given in his own words.

W. N.

Deſcription

VII.

Deſcription of a new Padlock of Security with Combinations. By CITIZEN REGNIER.*

THE intention of this padlock is to ſecure portmanteaus, cloak bags, and other packages in the moſt complete manner, and to ſerve occaſionally as defences to the key-holes of the doors of apartments. Deſcription of a padlock of combination.

The padlock is compoſed of four circular pieces of braſs, on which are engraven the twenty-four letters of the alphabet. The four pieces are moveable on their axes by turning them with the finger in order to produce the combination by which it is opened.

The combination of the manufacturer is the word ROME: when this word is brought into a correct line with the two marks on the edges of the two ſteel plates FF, which form the external part of the padlock, thoſe two plates can be ſeparated a little from each other, and the claſp of the lock can be opened by the hinge.

The ſame proceſs is uſed to faſten it, with this difference, that the two external plates are preſſed together ſo as to confine the bow or claſp of the lock in its cell at G; after which, the combination is to be ſhifted ſo that the characters ſhall no longer form the ſame word in the before-mentioned line.

The Method by which the Poſſeſſor may diſpoſe the Padlock to act by a new Combination, which cannot be known to any other Perſon:

1. A ſcrew is taken out, which paſſes through the centre of the plates FF.

2. The combination which it is intended to ſet aſide, namely, that which opens the lock at preſent muſt be duly arranged.

3. The marked circular pieces or rings muſt be taken off from four plates of braſs which conſtitute the central part, and together form the centre-piece of the mechaniſm.

4. Laſtly, The rings muſt be replaced on the centre pieces, each according to the letter the poſſeſſor may have choſen.

For example: If you would adopt the word LOCK for the combination, the letter L of the firſt ring muſt be placed over

* Tranſlated from a paper circulated by the conſtructor.

or

Deſcription of a padlock of combination.

or upon a ſmall ſteel tooth, which is attached to the firſt interior cylinder; the letter O of the ſecond ring on the tooth of the ſecond cylinder; the letter C of the third ring on the tooth of the third cylinder; and laſtly, the letter K of the fourth tooth on the fourth cylinder.

By this means the word *lock* is ſet up and becomes the combination of the lock, and the word *Rome* no longer produces the diſpoſition of parts required for the diſengagement.

After this operation the ſcrew muſt be replaced in the centre of the plate; this ſcrew does not contribute to the ſtrength of the mechaniſm; but is uſed merely to allow the exact ſpace neceſſary for opening the padlock, and to prevent the ſeparation of the rings from the central parts in the common uſe of the lock.

Method of uſing this Lock as a Defence to the Key-hole of a Door.

A ring ſtaple A, having a wood ſcrew, is fixed to the door above the key-hole or eſcutcheon of the lock.

A ſecond ring C is fixed perpendicularly beneath the other.

A cylindrical tube of iron D, in the form of a bolt, is placed vertically in the ring of theſe ſcrew ſtaples. At the lower end of the tube is an aperture, through which the padlock is inſerted, ſo that the tube or bolt cannot be raiſed or taken out.

By this contrivance the key-hole of the lock is completely defended, and the introduction of a pick-lock or falſe key is rendered morally impoſſible. For the mechaniſm preſents 331,776 combinations, * forming 331,775 different obſtacles, to prevent the removal of this defence by any perſon unacquainted with the ſecret of the proprietor.

If it be apprehended that the word of the combination may be forgotten, it will be eaſy to write and diſguiſe it in many different ways, without any riſk of diſcovery: for example,

	£.
The letter L, or eleventh letter of the alphabet will be written	11.
The letter O, or fourteenth letter - - - - -	14.
The letter C, or third letter - - - - - -	3.
The letter K, or tenth letter - - - - -	10.
Total (expreſſed)	£. 38.

* Number equal to the 4th power of 24.

This

This little calculation will appear to any other perſon to be a common account, but it is to the proprietor a memorandum by which he will perfectly recollect that the firſt letter of his combination is the eleventh of the alphabet, that the ſecond is the fourteenth, and ſo of the reſt. Deſcription of a padlock of combination.

REMARKS.

This padlock appears at firſt ſight ſimilar to that contrived by Cardan; but we know that his was not capable of having its combinations changed; whence it reſults that the manufacturer, the retailer, and every other perſon who may have ſeen it opened, can themſelves open it with the ſame facility as the proprietor himſelf. The notches which produce the opening may be alſo diſcovered by the feel; our lock has falſe notches cut in the centre-piece of the mechaniſm which prevent the diſcovery of the real one.

Laſtly, The claſp of this padlock is made of hardened and tempered ſteel, to prevent its being eaſily cut by an ordinary file. It is annealed ſo far only as to prevent its breaking.

ANNOTATION. *W. N.*

The remaining part of this paper contains the addreſs of the inventor and vendor, C. Regnier, *ci devant Jacobins, Rue Dominique, F. St. Germain à Paris,* and alſo a certificate of honourable mention, &c. from the *Athenée des Arts.*

The lock of Cardan conſiſts of the four viſible circular parts carrying the alphabet. Theſe as well as the central parts of the preſent lock are perforated half way through their centers by an hole, and quite through by a ſmaller hole, in the ſide of which laſt there is a notch extended to the circumference of the larger hole. All the four pieces are placed upon a central pin, which has ſide projections anſwering reſpectively to the notches, but occupying the ſpace of the larger hole while the lock is cloſed. From this conſtruction it is evident that the lock cannot be opened unleſs every one of the notches be placed oppoſite its projection; that this poſition or placing is ſettled by the maker, and not variable; and that the lock is liable to be opened, though not eaſily, by the tentative proceſs deſcribed at p. 204 of our laſt volume.

Cit. Regnier has perfected the lock of Cardan by making the ſyſtem of the alphabet moveable with regard to the internal

Deſcription of a padlock of combination.

nal notch at the pleaſure of the poſſeſſor, and alſo by making groves or ſmall notches on the face of each central piece, which anſwer the purpoſe of the teeth recommended at the page laſt quoted, by preventing the rings from being turned round while any pull is made againſt the cloſure.

I find ſome obſcurity in his deſcription of the manner of connecting the central piece and the external engraved part. From the operation, I apprehend, 1. that each ring has a number of notches at its inner ſurface, that anſwer to the letters on its outer face; 2. that each central round piece fits the cavity of its ring, and is prevented from turning by a tooth which it lodges in one of the notches; 3. that when all the four teeth are ranged in a line between F and F, the lock will open; and therefore, 4. when any particular letter is placed over the tooth, that letter becomes the effective letter for its own ring.

VIII.

Obſervations on the Quantity of horizontal Refraction; with a Method of meaſuring the Dip at Sea. *By* WILLIAM HYDE WOLLASTON, *M. D. F. R. S.**

Former paper of the author, and of Monge, upon horizontal refraction.

IN a Paper which I ſome time ſince preſented to this Society, (printed in the Phil. Tranſ. for 1800,) I endeavoured to aſcertain the cauſes, and to explain the various caſes, of horizontal refraction, which I had either obſerved myſelf, or had ſeen deſcribed by others.

At the time of writing that eſſay, I had not met with the *Mémoires ſur l'Egypte*, publiſhed but a ſhort time before; and I was not aware that an account had been given by M. Monge, of the phenomenon known to the French by the name of *mirage*, which their army had daily opportunities of ſeeing, in their march through the deſerts of Egypt.

In the peruſal of this memoir, I could not fail to derive inſtruction from the information it contained; but, as the facts related by him accord entirely with the theory that I had advanced, I was by no means induced to adopt the explanation that he has propoſed, in preference to my own.

* From the Philoſophical Tranſactions for 1803.

Objection to the theory of Monge.

The definite reflecting ſurface which he ſuppoſes to take place between two ſtrata of air of different denſity, is by no means conſiſtent with that continued aſcent of rarefied air which he himſelf admits; and the explanation founded on this hypotheſis will not apply to other caſes, which may all be ſatisfactorily accounted for, upon the ſuppoſition of a gradual change of denſity, and ſucceſſive curvature of the rays of light by refraction.

The ſubject well treated by Woltman.

I have ſince learned that the ſame ſubject has alſo been ably treated by Mr. Woltman, in Gilbert's *Annalen der Phyſik*; but I have to regret that his diſſertation, as well as that of Gruber, in the ſame Annals, were written in a language that was unknown to me, and that I could not avail myſelf of the aſſiſtance that I might otherwiſe have received from their reſearches.

Obſervations over the ſurface of the Thames.

When I formerly engaged in this inquiry, being impreſſed with the advantage to be derived from it to nautical aſtronomy, on account of the variations in the dip of the apparent horizon, from which all obſervations of altitude at ſea muſt neceſſarily be taken, I ſuggeſted the expediency of a ſeries of obſervations, to be made by a perſon attentive to thoſe changes of temperature or moiſture of the atmoſphere, on which he might find the depreſſion of his horizon principally to depend. I had at that time no expectation that I could myſelf purſue this ſubject farther to any uſeful purpoſe, having little proſpect of reſiding for a ſufficient length of time in view of the ſea, and ſeeing no other method by which the ſame end might be accompliſhed. I have, however, ſince that time, found means to ſatisfy myſelf, by obſervations over the ſurface of the Thames, that although the quantity of refraction varies in general with any change of the thermometer or hygrometer, yet the law of theſe variations is not altogether ſo ſimple as I had hoped it might be found.

Narrative.

I ſhall, on the preſent occaſion, firſt relate the facts on which this opinion is founded, and which are in themſelves ſufficiently remarkable, on account of the unexpected quantity of refraction obſervable over a ſhort extent of water; I ſhall, in the next place, ſhew that the exact determination of the concurrent changes of the atmoſphere are of leſs value, and their irregularities of leſs conſequence, than I had conceived, as there is a very eaſy method whereby the quantity of dip at ſea may be at any time correctly meaſured; and therefore the end which I ſought by indirect means, may be at once directly attained.

The

Apparent diſtortion of the parts of veſſels ſeen over the ſurface of the Thames.

The firſt inſtance that occurred to me, of obſervable refraction over the ſurface of the Thames, was wholly accidental. I was ſitting in a boat near Chelſea, in ſuch a poſition that my eye was elevated about half a yard from the ſurface of the water, and had a view over its ſurface, that probably ſomewhat exceeded a mile in length, when I remarked that the oars of ſeveral barges at a diſtance, that were then coming up with the tide, appeared bent in various degrees, according to their diſtance from me. The moſt diſtant appeared nearly in the form repreſented, Plate I. Fig. 1. *d d* being my viſible horizon by apparent curvature of the water; *a b* the oar itſelf in its inclined poſition; and *b c* an inverted image of the portion *b e*. By a little attention to other boats, and to buildings on ſhore, I could diſcern that the appearance of all diſtant objects ſeen near the ſurface of the water was affected in a ſimilar manner, but that ſcarcely any of them afforded images ſo perfectly diſtinct as the oblique line of an oar dipped in the water.

Theſe effects are different from what might be cauſed by reflection.

A perſon preſent at the time (as well as ſome others to whom I have ſince related the circumſtance) was inclined to attribute the appearance to reflection from the ſurface of the water; but, by a moderate ſhare of attention, a very evident difference may be diſcovered between the inverſion occaſioned by reflection, and that which is cauſed by atmoſpherical refraction. In caſes of reflection, the angles between the object and image are ſharp, the line of contact between them ſtraight and well defined, but the lower part of the image indefinite and confuſed, by means of any ſlight undulation of the water. But, when the images are cauſed by refraction, the confines of the object and its inverted image are rounded and indiſtinct, and the lower edge of the image is terminated by a ſtraight line at the ſurface of the water. In addition to theſe marks of difference, there is another circumſtance which, if attended to, muſt at once remove all doubt; for, by bringing the line of ſight near to the ſurface of the water, boats and other ſmall objects are found to be completely hidden by an apparent horizon, which, in ſo ſhort a diſtance, cannot be owing to any real curvature of the water, and can ariſe ſolely from the bending of the rays by refraction.

They appear referrible to an higher temperature in the

When I reflected upon the cauſes which were probably inſtrumental in the production of theſe phenomena, they appeared referrible

referrible to difference of temperature alone. After a ſucceſſion of weather ſo hot that the thermometer, during one month preceding, had been 12 times above 80°, and on an average of the month at 68°, the evening of that day (Auguſt 22, 1800) was unuſually cold, the thermometer being 55°. The water might be ſuppoſed to retain the temperature it had acquired during a few weeks preceding, and, by warming the ſtratum of air immediately contiguous to it, might cauſe a diminution of its refractive denſity, ſufficient to effect this inverted curvature of the rays of light, in the manner formerly explained. As I was at that time unprovided with inſtruments of any kind, I had it not in my power to eſtimate the quantity of refraction, or temperatures; and can only ſay that, to my hand, the water felt in an uncommon degree warmer than the air.

water, which being communicated to the loweſt ſtratum of the air, alters its refractive power.

Being thus furniſhed with an unexpected field for obſervation, I from that time took ſuch opportunities as ſimilar changes of the weather afforded me, of examining and meaſuring the quantities of refraction that might be diſcovered by the ſame means over another part of the river, that I found moſt ſuited to my convenience.

Particular account of the obſervations.

The ſituation from which the greater part of my obſervations were made, was at the S. E. corner of Somerſet houſe. The view from this ſpot extends under Blackfriars bridge, towards London bridge, upwards of a mile in length, and in the oppoſite direction through Weſtminſter bridge, which is three quarters of a mile diſtant.

Such diſtances are however by no means neceſſary; and indeed the air over the river, in cold weather, is generally, or at leaſt very frequently, not ſufficiently clear for ſeeing diſtinctly to ſo great diſtances. For, ſince the winds which are moſt likely to effect a ſufficient change of temperature, on account of their coldneſs, are uſually from the E. or N. E. the principal ſmoke of the town is then brought in that direction, and hovers, like a denſe fog over the courſe of the river. This circumſtance deprived me of many opportunities which the changes of the thermometer indicated to be favourable for my purpoſe, and obliged me often to make uſe of ſhorter diſtances than I ſhould otherwiſe have choſen, by bringing the line of ſight as near as I could to the ſurface of the water.

For this purpoſe, I had a plane reflector fitted to the object-end of a ſmall pocket teleſcope, at an angle of 45°, ſo that,

when the telescope was held vertically, it gave a horizontal view at any level that was found most eligible. When the water has been calm, I have observed that the greatest refraction was visible within an inch or two of its surface, and I have then seen a refraction of six or seven minutes in the space of 300 or 400 yards: at other times, I have found it greatest at the height of a foot or two; but, in this case, a far more extensive view becomes necessary.

The first measures that I took were on the 23d of September, 1800. The water was $2\frac{1}{2}^{\circ}$ warmer than the air, and I found a refraction of about 4′.

Oct. 17. The difference of temperature was 3°, and the refraction 3′.

Oct. 22. The water was $11\frac{1}{2}^{\circ}$ warmer than the air, yet the quantity of refraction did not exceed 3′.

The smallness of the quantity of refraction upon this occasion, I attributed to the dryness of the atmosphere, conjecturing that a rapid evaporation might in great measure counteract that warmth which the water would otherwise have communicated to the air.

From that time, therefore, I have noted not only the heights of the thermometer in the water and in the air, but have added also the degrees of cold produced by keeping the bulb of it moistened for a sufficient time to render it stationary. In confirmation of my conjecture respecting the dryness of Oct. 22, I have also, in the following Table, which comprises the whole of my observations, inserted a column from the Register kept at the apartments of the Royal Society, containing the heights of the hygrometer, on those mornings when my observations were made.

TABLE.

TABLE.

Table of obſervations:

At 8, A. M.	Air.	Water.	Difference.	Refraction.	Cold by evaporation.	Hygrometer.
1800. Sept. 23	57	60½°	3½°	4′	— —	72°
Oct. 17	46½	49½	3	3	— —	72
22	38	49½	11½	3	— —	67
Nov. 1	41	45½	4½	8	½°	76
4	43¾	46¾	3	3 —	1¾	72
5	37	45	8	8 +	1	69
12	44½	48½	4	1 +	3½	73
13	40	44½	4½	5	½	76
1801. June 13	50	63	13	9 +	5	65
22	55	61	6	6 +	6	65
23	55	62	7	6	4½	65
24	55	61	6	5	3	67
Sept. 8	60	64	4	7	2	78
9	64	64¾	¾	5	3	74
10	58	64	6	7	2	70
12 o'clock, 10	63	64	1	2		

From a review of the preceding Table it will be found, upon the whole, that when the water is warmer than the air, ſome increaſe of depreſſion of the horizon may be expected; but that its quantity will be greatly influenced, and in general diminiſhed, by dryneſs of the atmoſphere.

The dip of the viſible horizon will be increaſed by the water being warmer than the air; and diminiſhed (more conſiderably) by dryneſs in the air.

It appears, however, that no obſervable regularity is deducible from the meaſures above given; but that the quantity, on ſome occaſions, is far different from what the ſtates of the thermometer and hygrometer would indicate. On the 9th of September, for inſtance, the difference of temperature is only ¾°, and the evaporation, to counteract this ſlight exceſs of warmth, produced as much as 3° of cold; nevertheleſs, the refraction viſible was full 5′. In this obſervation I think that I could not be miſtaken, as the water was at the time perfectly calm, the air uncommonly clear, and I had leiſure to pay particular attention to ſo unforeſeen an occurrence.

The meaſures indicate little of any practical law.

This one inſtance appears conformable to the opinion entertained by Mr. Huddart, and by M. Monge, that, under ſome circumſtances, the ſolution of water in the atmoſphere cauſes a decreaſe in its refractive power; but, on no other occaſion have I been induced to draw a ſimilar inference.

Solution of moiſture not of much probable conſequence.

The object that I have at all times chosen, as shewing best the quantity of refraction, has been either an oar dipped in the water at the greatest discernible distance, or some other line equally inclined; and the angle measured has been, from the point where the inverted image is terminated by the water, to that part of the oar itself which appears to be directly above it. (The apparent magnitude of *ec*, Fig. 1. Pl. 3.)

The eight first angles were taken with a mother-of-pearl micrometer in the principal focus of my telescope, and are not so much to be depended upon for accuracy as the succeeding eight. These last were measured with a divided eye-glass micrometer, and consequently are not liable to any error from unsteadiness of the instrument or object.

It is not likely that so great a variation of the dip happens out at sea.

From the foregoing observations we learn, that the quantity of refraction over the surface of water may be very considerable, where the land is near enough to influence the temperature of the air. At sea, however, so great differences of temperature cannot be expected; and the increase of dip caused by this variation of horizontal refraction, it is to be presumed, is not so great as in the confined course of a river; but, if we consider that it may also be subject to an equal diminution from an opposite cause, and that the horizon may even become apparently elevated, there can be no question that the error in nautical observations, arising from a supposition that it is invariably according to the height of the observer, stands in need of correction.

Mr. Huddart's remedy.

The remedy employed by Mr. Huddart,* of taking two angles of the sun from opposite points of the horizon at the same time, and considering the excess of their sum above 180° as double the dip, must without doubt be effectual; but, from causes which he assigns, it is practicable only within certain limits of zenith distance; for, where the zenith distance is small, and the changes of azimuth rapid, there is required considerable dexterity and steadiness of a single observer who attempts to turn in due time, from one observation to another; and, when it exceeds 30°. the greater angle cannot be measured with a sextant, and consequently his method is, with that instrument, of use only in low latitudes.

* Phil. Trans. for 1797, p. 40.

On

Objections to the back obſervation of angles,

On account of the difficulty attending ſome of the adjuſtments for the back obſervation, he rejects that method for taking angles in general, with much reaſon; but he has thereby overlooked a means of determining the dip, which I am inclined to think might be employed with advantage in all latitudes, without any occaſion to hurry the moſt inexperienced or cautious obſerver.

but the method recommended for meaſuring the arc between the oppoſite horizons.

By the back obſervation, the whole vertical angle between any two oppoſite points of the horizon may be meaſured at once, either before or after taking an altitude. Half the exceſs of this angle above 180°, ſhould of courſe be the dip required.

Error of the glaſſes found by reverſing the whole inſtrument,

But, if it be doubtful whether the inſtrument is duly adjuſted, a ſecond obſervation becomes neceſſary. The inſtrument muſt be reverſed, and, if the apparent deficiency of the oppoſite angle from 180° be not equal to the exceſs before obtained, the index error may then be corrected accordingly; and, ſince the want of adjuſtment, either of the glaſſes at right angles to the plane of the inſtrument, or of the line of ſight parallel to it, will affect both the larger and ſmaller angle very nearly in an equal degree, the $\frac{1}{4}$ part of their difference will be extremely near the truth, and the errors ariſing from want of thoſe adjuſtments may with ſafety be neglected.

as firſt ſuggeſted by Ludlam.

This method of correcting the index error for the back obſervation at ſea, was many years ſince recommended by Mr. Ludlam; * yet I do not find that it has been noticed by ſubſequent writers on that ſubject, or ſuggeſted by any one for determining the dip; but I can diſcover no reaſon for which it could be rejected as fallacious, and I ſhould hope that in practice it would be found convenient, ſince in theory it appears to be effectual.

Whether refraction may be different on different bearings at the ſame time.

The moſt obvious objection to this, as well as to Mr. Huddart's method, is the poſſibility that the refraction may be in ſome meaſure different in oppoſite points of the horizon at the ſame time. When land is at no great diſtance, ſuch an inequality may be found to occur; but, upon the ſurface of the ocean in general, any partial variations of temperature can rarely be ſuppoſed to exiſt; and it is probable, that under any circumſtances, the difference will not bear any conſiderable

* Directions for the uſe of Hadley's quadrant, 1771, § 82, p. 56.

proportion

proportion to the whole refraction; nor can it be thought a ſufficient reaſon for rejecting one correction propoſed, that there may yet remain other ſmall errors, to which all methods are equally liable, but which it is not the object of the preſent diſſertation to rectify.

IX.

An Account of Two Halos, with Parhelia. *By Sir* H. C. Englefield, *Bart.* F. R. S*.

Uncommon halos round the ſun.

On the 20th of November, 1802, at two o'clock in the afternoon, going out of doors, at Richmond in Surry, I perceived the ſun, accompanied by uncommon halos and parhelia. The weather was ſhowery, and the ſky had that peculiar turbid appearance, which is the certain forerunner of heavy and long continued rain. The ſun ſhone with a faint and watery light, was very ill defined, and appeared rather elongated in a vertical direction. A very denſe cloud occupied all that quarter of the horizon, and roſe up pretty near to the ſun. Very heavy clouds covered the eaſtern part of the heavens, extending quite to the north, and were proceeding gradually towards the ſouth weſt. The wind was about eaſt.

The altitude of the ſun was 14°. The circle neareſt the ſun was diſtant from him nearly 21°, and was about a degree in breadth. It was of a pale yellowiſh light, but had no tendency to priſmatic colours.

On the left hand, it extended below a line drawn through the ſun parallel to the horizon. To the right, it terminated in denſe clouds conſiderably above that line.

The exterior circle was 48° from the ſun, and it might be $1\frac{1}{2}$° in breadth, as it was evidently broader than the inner circle. It terminated on either hand at nearly the ſame height as the interior one. It was tinged throughout with the priſmatic colours, though only red, green, and blue, were diſtinctly viſible. The red was neareſt the ſun. The blue very faint. The brightneſs of this circle was about that of the ſecondary rainbow, to a bright common bow.

* Royal Inſtitution, II. 1.

In

In a line parallel to the horizon, paſſing through the ſun, there was, in the left hand branch of the inner circle, a very faint parhelion; but in the upper point of the ſame circle, there was a very bright and remarkable one. Its light was ſo vivid, that it could ſcarcely be ſteadily viewed; and, indeed, it was rather brighter than the real ſun. It was of a whiter light than the reſt of the circle in which it was, and had a pearly appearance, as partaking a little of priſmatic tints. It was large, perhaps in its brighteſt part near two degrees broad, very ill defined every where, but moſt diffuſed in the part furtheſt from the ſun. From each ſide of the bright light proceeded a bright ray, which had a double curvature very diſtinct, being firſt convex towards the ſun, and then concave. The lower edge of theſe rays (or that neareſt the ſun), was tolerably well defined, the upper edge melted away into the ſky, with a ſort of ſtreakineſs. They grew both narrower and fainter towards their termination, and they reached pretty near to the other circle. Uncommon halos round th ſun.

The whole form of this parhelion and its rays, bore ſo ſtriking a ſimilitude to the body and extended wings of a long winged bird, ſuch as an eagle, hovering directly over the ſun, that ſuperſtition would really have had little to add to the image.

There was no trace of any other circle or arch in the heavens, nor of any anthelion.

It is probable, that it had been ſtill more beautiful before I ſaw it, as during the time I obſerved it, its brightneſs was continually diminiſhing; ſome traces, however, were viſible for nearly half an hour.

The meaſures which I have given muſt be conſidered as very rough. I had no inſtrument at hand, but a ſix inch pocket ſector. I held the joint of this as cloſe to my eye as I could, and opened it, till the points of the legs coincided with the ſun and with the circles that I wiſhed to meaſure. I am, however, inclined to think, that the meaſures I have given are true within a degree.

The accompanying ſketch, (Plate III.) which is drawn on a ſcale of 20° to an inch, from a rough draught which I made at the moment, will give a more diſtinct idea of the whole appearance than can be conveyed by words.

A Theory

A Theory of Halos and Parhelia. *By* THOMAS YOUNG, M. D. F. R. S.

Theory of halos and parhelia.

The explanation of the primary and ſecondary rainbow begun by De Dominis, and completed by Deſcartes and Newton, derives an entire and ſatisfactory confirmation, from the perfect coincidence of the obſerved angular magnitudes, with the reſult of calculations of the effect of ſpherical drops. We know that drops of water, either accurately, or very nearly ſpherical, exiſt in great abundance in every cloud, and in every ſhower of rain; and whatever their dimenſions may be, they muſt neceſſarily conſpire in the ſame general effect, of producing the ſame rainbow, whenever a ſpectator is placed in a proper ſituation for obſerving it; conſequently ſuch rainbows are of very frequent occurrence.

Variable halos produced by equal drops.

I have attempted to ſhow, that for producing the phenomena of variable halos, often obſervable in hot climates, it is only neceſſary that a conſiderable part of the ſpherules of a cloud or miſt, be either accurately, or very nearly, of equal magnitude, a condition, of which the poſſibility is eaſily admitted from analogy, and the probability is favoured by the apparent uniformity of the different parts of ſuch miſts as we can examine.

The conſtant halo of 23° not explained.

But no ſatisfactory reaſon has hitherto been aſſigned for the production of the halo, which in theſe climates is the moſt common of all; that is, the conſtant halo of 23° or 24°. The hypotheſis by which Huygens attempted to explain the production of halos and parhelia, are both arbitrary and improbable. He imagined the exiſtence of particles of hail, ſome globular, others cylindrical, with an opaque part in the middle of each, bearing a certain ratio to the whole; and he ſuppoſed the poſition of the cylinders to be ſometimes vertical, and ſometimes inclined to the horizon in a given angle.

The hypotheſis of Huygens improbable.

It has already been objected, that no ſuch particles have ever been obſerved to accompany halos; and it is, beſides, highly improbable, that ſuch an opaque part ſhould bear the ſame proportion in all the hailſtones, and that the cylinders ſhould have terminations ſo peculiar as is ſuppoſed; and the moſt incredible circumſtance of all is, that all theſe proportions ſhould be conſtantly ſuch, as always to produce a halo at the diſtance of 23° or 24° from the ſun or moon.

It

It appears, that a much ſimpler and more natural explanation of theſe phenomena may be deduced from the regular cryſtallization of ſnow in the atmoſphere.

It is well known, that the cryſtals of ice and ſnow, tend always to form angles of 60° ; now a priſm of water or ice, of 60°, produces a deviation of 23° 37′, for rays forming equal angles with its ſurfaces, and the angle of deviation varies at firſt very ſlowly, as the inclination changes, the variation amounting to leſs than 3°, while the inclination changes 30°.

The equilateral priſmatic cryſtals of ice produce a deviation of 23° 37′ which varies at firſt very ſlowly.

Now if ſuch priſms were placed at all poſſible angles of inclination, differing equally from each other, one half of them would be ſo ſituated, as to be incapable of tranſmitting any light regularly by two ſucceſſive refractions directed the ſame way ; and of the remaining two fourths, the one would refract all the light within theſe three degrees, and the other would diſperſe the light in a ſpace of between 20° and 30° beyond them.

The caſual arrangement of theſe will give the halo.

In the ſame manner, we may imagine an immenſe number of priſmatic particles of ſnow to be diſpoſed in all poſſible directions, and a conſiderable proportion of them to be ſo ſituated, that the plane of their tranſverſe ſection may paſs within certain limits of the ſun and the ſpectator. Then half of theſe only will appear illuminated, and the greater part of the light will be tranſmitted by ſuch as are ſituated at an angular diſtance of 23° 37′, or within 3° of it : the limit being ſtrongly marked internally, but the light being externally more gradually loſt. And this is preciſely the appearance of the moſt common halo. When there is a ſufficient quantity of the priſmatic particles, a conſiderable part of the light muſt fall, after one refraction, on a ſecond particle ; ſo that the effect will be doubled : and, in this caſe, the angle of refraction will become ſufficient to preſent a faint appearance of colour, the red being internal, as the leaſt refrangible light, and the external part having a tinge of blue.

Refraction through two priſms gives the greater halo of 47°.

Theſe concentric halos of $23\frac{1}{2}$° and 47°, are therefore ſufficiently explicable, by particles of ſnow, ſituated promiſcuouſly in all poſſible directions. If the priſms be ſo ſhort as to form triangular plates, theſe plates, in falling through the air, will tend to aſſume a vertical direction, and a much greater number of them will be in this ſituation than in any other. The reflection from their flat ſurfaces will conſequently produce a horizontal circle of equal height with the ſun ; and their refraction will

Very ſhort priſms or plates will fall edgewiſe ; and the reflection of their ends will give the horizontal circle, and their reflection a parhelion with wings.

will exhibit a bright parhelion immediately over the ſun, with an appearance of wings, or horns, diverging upwards from the parhelion.

Exp. with the priſm.

For all ſuch particles as are directed nearly towards the ſpectator, will conſpire in tranſmitting the light much more copiouſly than it can arrive from any other part of the circle; but ſuch as are turned more obliquely, will produce a greater deviation in the light, and at the ſame time a deflection from the original vertical plane. This may be eaſily underſtood, by looking at a long line through a priſm, held parallel to it: the line appears, inſtead of a right line, to become a curve, the deviation being greater in thoſe rays that paſs obliquely with reſpect to the axis of the priſm; which are alſo deflected from the plane in which they were paſſing.

The line viewed through the priſm has no point of contrary flexure, but if its ordinates were referred to a centre, as in the caſe of the halos, it would aſſume a form ſimilar to that which Sir Henry Engleſield has deſcribed.

Tho' ſnow flakes are complicated, their elements may be ſimple in the upper regions, &c.

The form of the flakes of ſnow as they uſually fall, is indeed more complicated than we have been ſuppoſing, but their elements in the upper regions of the air are probably more ſimple. The coincidence in the magnitude of the obſerved and calculated angles is ſo ſtriking, as to be nearly deciſive with reſpect to halos, and it is not difficult to imagine that many circumſtances may exiſt, which may cauſe the axis of the greater number of the priſms to aſſume a poſition nearly horizontal, which is all that is required for the explanation of the parhelia with their curved appendages. Perhaps alſo, the effect may ſometimes be facilitated by the partial melting of the ſnow into conoidal drops: for it may be ſhown, by the light of a candle tranſmitted through a wine glaſs full of water, that ſuch a form is accommodated to the production of an inverted arch of light, like that which is frequently obſerved to accompany a parhelion.

A Deſcription

X.

A Deſcription of Dr. Young's *Apparatus for illuſtrating the Doctrine of Preponderance* *. Plate I. Fig. II.

Apparatus of pullies for ſhewing the moſt advantageous employment of force.

ALTHOUGH there can be no doubt of the truth of the mathematical concluſions, which have been deduced from the well known laws of motion, reſpecting the moſt advantageous employment of force in machines, yet they have, in general, been too little conſidered in practical works, and ſcarcely ever enforced by experimental illuſtration. The apparatus contrived for this purpoſe, has been mentioned in the account of the lectures on mechanics; its advantage is derived from the ſimplicity of its operation, and the facility of obſerving at once the ſeveral motions, which begin at the ſame time, and may eaſily be compared, as long as they continue. The ratio of the portions of the middle pulley, which is that of 5 to 2, is near enough to the maximum $(\sqrt{2} \times 1) : 1$; and the other ratios 3 : 2 and 4 : 1 are taken ſufficiently different from this to ſhow that the velocity of each is inferior to that of the middle pulley. The pulleys are all perforated in the axis, and move freely on a ſtrong poliſhed wire, ſupported by two ſhort arms, projecting a little from two upright pieces about three feet in length, in order that the deſcending weights may proceed without interruption beyond the edge of the table.

An Account of an Experiment on the Velocity of Water flowing through a Vertical Pipe. By the ſame Author.

Whether the diſcharge of water through a vertical pipe from the bottom of a ciſtern be the ſame as from an hole in a ciſtern of the total depth.

IT has been aſſerted by ſome writers on hydraulics, and Venturi deſcribes a particular experiment in ſupport of the aſſertion, that the diſcharge of water running out of the bottom of a ciſtern, through a deſcending pipe, is nearly the ſame as if the ciſtern were continued through the whole height, from the ſurface of the water to the orifice of the pipe, and the water were then diſcharged from the bottom of the ciſtern by a ſhort pipe in any direction. The apparent difficulty of finding a cauſe adequate to the effect, on the one hand, and the

* In the lectures of the Royal Inſtitution from whoſe Journal No. 11. the preſent and next articles are taken.

authority

authority of Venturi on the other, made it desirable that the experiment should be repeated; and an apparatus Fig. III. Plate I. was constructed, in the house of the Royal Institution, for performing it in a simple and satisfactory manner. The cistern employed was a cube of nine inches: close to the bottom a cylindrical tube was inserted, in a horizontal direction, nine inches in length, and half an inch in diameter; another tube, of exactly the same dimensions, was provided with a flat funnel at its upper end, and its lower end was fitted to slide in a collar placed in one of the upper angels of the cistern, so that it was supported in a vertical position. Water was poured into the funnel, as fast as it could be transmitted through the tube, and, as the surface of the fluid rose in the cistern, the vertical tube was drawn up, so that its lower orifice was barely immersed in the water. It was expected, that if the velocity of the water in the vertical tube were equal to the velocity corresponding to half its length, the water in the cistern would stand at the height of four inches and a half, or one half of that length, and that the pressure of this head of water would generate, in the water flowing through the horizontal tube, nearly the same velocity as the column of water would acquire in its descent through the vertical tube: the friction and resistance being in both cases the same.

Experiment shewed the contrary.

But the result was far different, and it fully confirmed the truth of the received theory: for the water rose in the cistern to the height of eight inches, which was very nearly the length of the tube. It is true that the water had already some velocity when it entered the funnel; but most of this must have been lost by reflection from its sides and bottom; and the quantity of air bubbles, that were unavoidably carried down with the water, must have fully compensated the little that remained.

The entire adhering column in the tube renders the atmosphere active upon the surface of the fluid, and produces the same effect as an head of that height.

It appears therefore, that we are to consider this effect in a light somewhat different from that in which it was placed in the lectures on hydraulics. The water acquires all its velocity, in consequence of the pressure of the atmosphere acting jointly with its cohesion, in a very small space at the entrance of the tube: consequently, during the whole time of its descent it acquires no new motion, and the whole force of its gravitation must therefore be at liberty to act in any other way; hence the whole column produces the same degree of pressure as if it were at rest, and causes the atmosphere to press on the water above

above it in proportion to its whole height, in the ſame manner as if the preſſure were derived in any other way from an equal column of water; and the caſe is reduced to a perfect analogy with the preſſure of a head of water of this height, ſince the air acts upon the particles entering the tube in the ſame manner as the water does in more common caſes. Had the reſult of the experiment been different, it would have been an exception to the general principle of the preſervation of living force, or the equality of the potential aſcent to the actual deſcent; for, the water moving with the velocity due to half the height only, would have been capable of aſcending but to half the height.

XI.

Account of a ſimple Eudiometric Apparatus conſtructed and uſed by Dr. T. C. Hope, *F. R. S. Edin.* &c. &c.

The uncertainty of eudiometrical experiments with nitrous gas renders the uſe of other ſubſtances neceſſary.

SINCE the diſcovery of the uncertainty with which the application of nitrous gas to atmoſpheric air, and other mixtures containing oxigen is attended, it has been found deſirable to preſent ſolid or liquid ſubſtances for the abſorption of that principle. This on firſt conſideration may ſeem at leaſt as eaſy to be done as to mix two gaſes; but it is by no means ſo, becauſe the liquids in particular poſſeſs a degree of chemical activity which readers it inconvenient to immerſe the hands in them, or to expoſe their ſurface to the open air. Dr. Hope, whom I had lately the pleaſure of ſeeing in town, mentioned an apparatus he uſes in his lectures and experiments, which is at once ſimple and effectual, and I am happy in his permiſſion to deſcribe it in this place.

Apparatus by which fluids can be applied to air without inconvenience.

A Fig. 3. Plate IV. repreſents a bottle which may be $1\frac{1}{2}$ inch in diameter, and $2\frac{1}{2}$ inches in length, having a neck and ſtopper at D, and another neck as uſual at C, into which laſt the neck of the bottle or body B, is fitted by grinding. This laſt was made of the ſame diameter as the bottle, but ſeven or eight inches long. B contains the gas, and A the liquid; for example, ſolution of hydroſulphuret. When B is thus connected with A, the compound veſſel may be inverted and agi-

tated; and the liquid will flow into B, where it will abſorb the oxigen, and form a partial vacuum. If this circumſtance be found, or apprehended, to prevent the complete or rapid abſorption, the veſſel A may be plunged beneath the ſurface of common water, and the ſtopper D ſlowly opened. The preſſure of the atmoſphere will then force in a quantity of water, which will dilute the hydroſulphuret, but not ſufficiently to prevent the completion of the proceſs. The veſſel B muſt be graduated to ſhow the dimenſions of the reſidue, or otherwiſe this reſidual gas may be transferred into a veſſel expreſsly graduated for meaſuring gaſes.

The apparatus ſimply and highly convenient.

By this ſimple and elegant apparatus we ſee that the liquid is economized, and the facility, neatneſs, and preciſion of experiment inſured. The ſize here mentioned is very well adapted to the purpoſes of public demonſtration; but it is almoſt needleſs to remark, that it may be made conſiderably ſmaller without depriving it of its utility and excellence.

P. S. While reading this proof, I have received a line from Dr. Hope, by which I am very ſorry to find that the ſketch I have given is not accurate; but as it is now late in the month, and the figure is engraved, I ſhall be careful to give another engraving with the obſervations he may favour me with. W. N.

SCIENTIFIC NEWS.

*Combuſtion of Metals in non-reſpirable Gaſes, by means of Galvaniſm.**

Metals burned in nitrogen, &c. by galvaniſm.

PROFESSOR Tromſdorff has noticed that metals are combuſtible by means of the galvanic ſpark in hidrogen, ammonia, nitrogen, nitrous and carbonic acid gaſes.

Reduction of the Oxide of Titanium.

Reduction of titanium.

Profeſſor Lampadius has ſucceeded in reducing to the metallic ſtate by means of charcoal only, the oxide of titanium, obtained by decompoſing the gallate of titanium by potaſh or ſoda. The metallic titanium is of a dark copper colour; it

* Tromſdorff's Hiſtory of Galvaniſm and its chemical agency, p. 122.

has

has much metallic brilliancy, is brittle, and poſſeſſes in ſmall ſcales a conſiderable degree of elaſticity. It tarniſhes on expoſure to air, and becomes eaſily oxided by heat. It then acquires a blueiſh aſpect. It detonates with nitrate of potaſh, and is highly infuſible. All the denſe acids act upon it with conſiderable energy. *Scherer's Journ.* IX. p. 49. p. 72.

On the Precipitability of the Oxide of Biſmuth.

Solution of biſmuth by heat not precipit. by water.

Mr. Buckholtz has found that the ſolution of biſmuth prepared *in the cold* is alone decompoſable, by a copious addition of water, but that no ſuch effect takes place in the ſolution prepared *by means of heat*. He has alſo noticed that a ſolution of this metal prepared in thec olde, depoſits its oxide in a cryſtalline form merely by warming the ſolution gradually.

Scherer IX. p. 73.

New Method of preparing phoſphate of Soda.

Preparation of phoſphate of ſoda.

Mr. Funcke, apothecary at Linz, in Germany, has diſcovered a new method of preparing phoſphate of ſoda, in a more œconomical, expeditious and eaſy manner, than any of the proceſſes hitherto made uſe of by manufacturers or chemiſts. His proceſs conſiſts, in ſaturating the exceſs of lime contained in calcined bones with dilute ſulphuric acid, and then diſſolving the remaining phoſphate of lime in nitric acid. To this ſolution, he adds a like quantity of ſulphate of ſoda, and then recovers the nitric acid by diſtillation. The phoſphate of ſoda is then ſeparated from the ſulphate of lime, by the affuſion of water, and cryſtallization in the uſual manner. *Scherer* IX. 59.

REMARK BY THE TRANSLATOR. (A.)

This proceſs ſeems to be much preferable to that now in uſe, *viz.* to decompoſe the bones of animals burned to whiteneſs, by ſulphuric acid, and then preſenting ſoda to the diſengaged phoſphoric acid. For the phoſphate of lime cannot be completely decompoſed by the affuſion of ſulphuric acid; on account of this acid forming inſtantly a portion of ſulphate of lime: the liberated phoſphoric acid then produces with the remaining undecompoſed portion of phoſphate of lime, a ſub-phoſphate of lime, which cannot be decompoſed by ſulphuric acid, and which together with the ſulphate of lime already produced, forms an

unmanageable and bulky maſs. And again if to this maſs, waſhed out with water as well as poſſible, carbonate of ſoda be now preſented, a partial decompoſition will only be effected, for it is the exceſs of the phoſphoric acid of this ſalt only, which in that caſe forms the article ſought. The remaining portion of phoſphoric acid remain united to the lime, in the form of phoſphate of lime. The above proceſs is therefore evidently better.

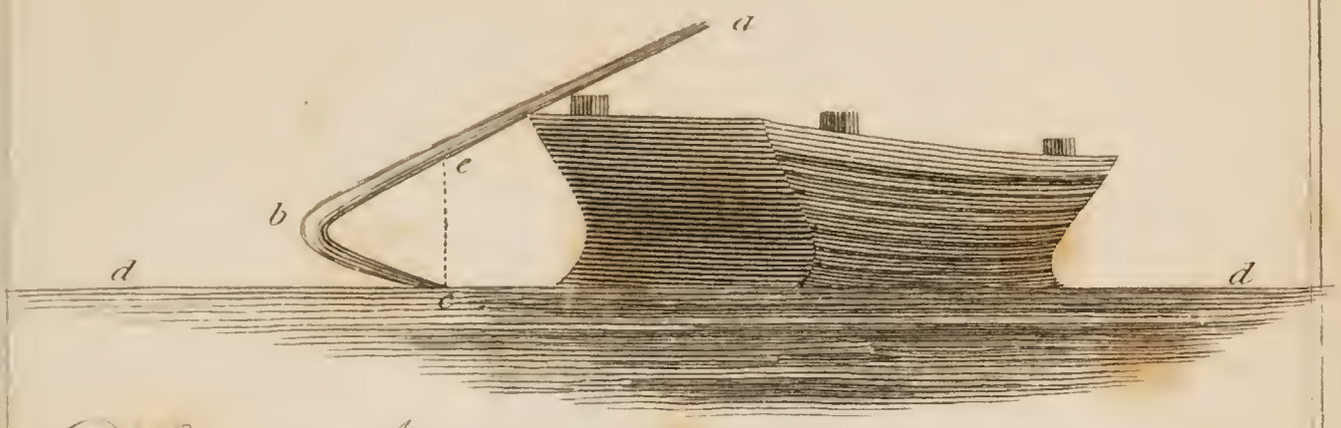
Sulphate of Soda prepared from Sulphate of Lime.

Sulphate of ſoda from gypſum.

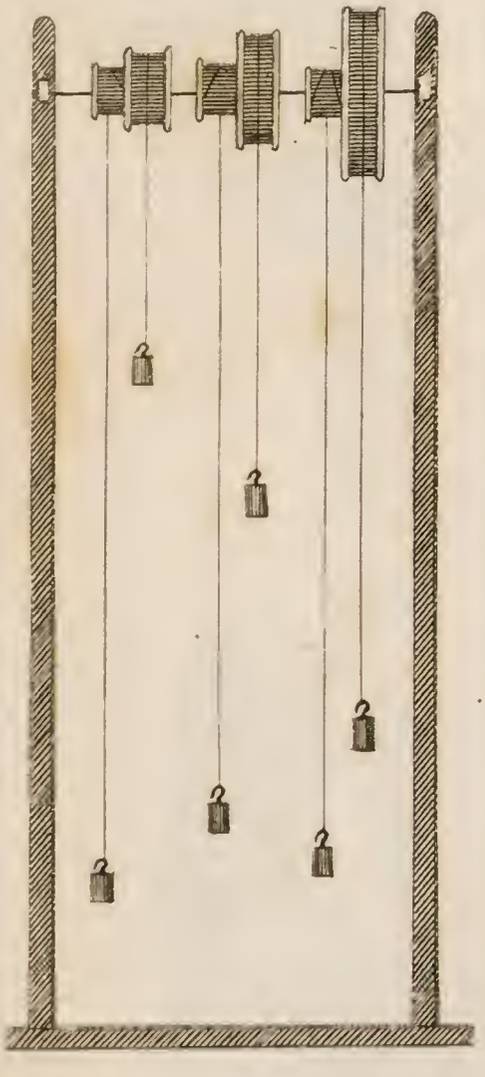
This method conſiſts in making into a paſte with a ſufficient quantity of water, eight parts of burned gypſum, or ſulphate of lime, five of clay, and five of common ſalt. This mixture is burned in a kiln or other convenient oven, and then ground to powder, diffuſed in a ſufficient quantity of water, which after being ſtrained and evaporated, is ſuffered to cryſtallize.

Scherer IX. 61.

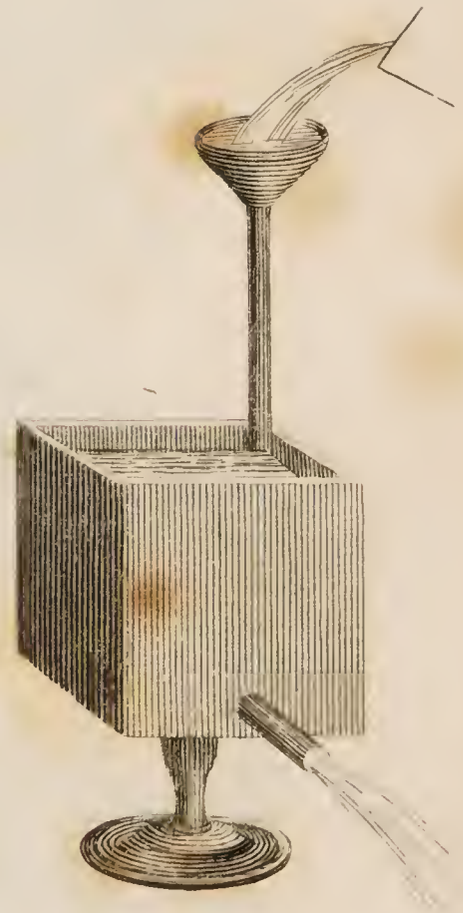
D.r Wollaston's on Terrestrial Refraction.



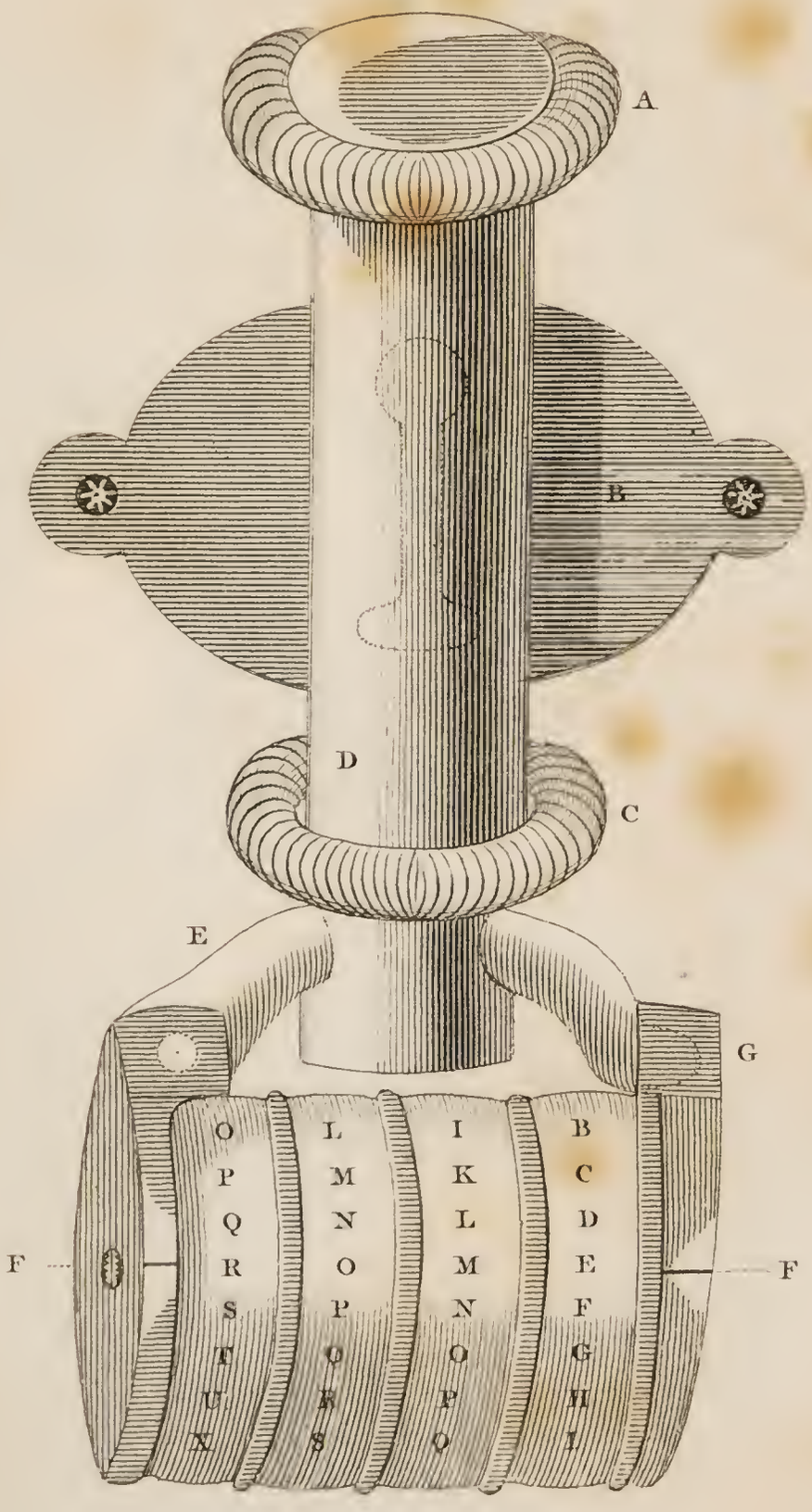
D.r Young's Apparatus to Illustrate the Doctrine of Preponderance.



Experiment of the Velocity of Water thro a Vertical Pipe.



Lock of Combination by M. Regnier.

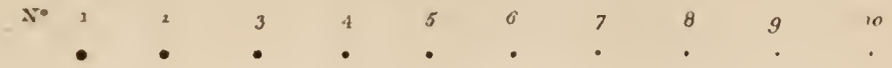


Tiffany Sc. Ky. Bell Co.

Mallon & Russell Co.

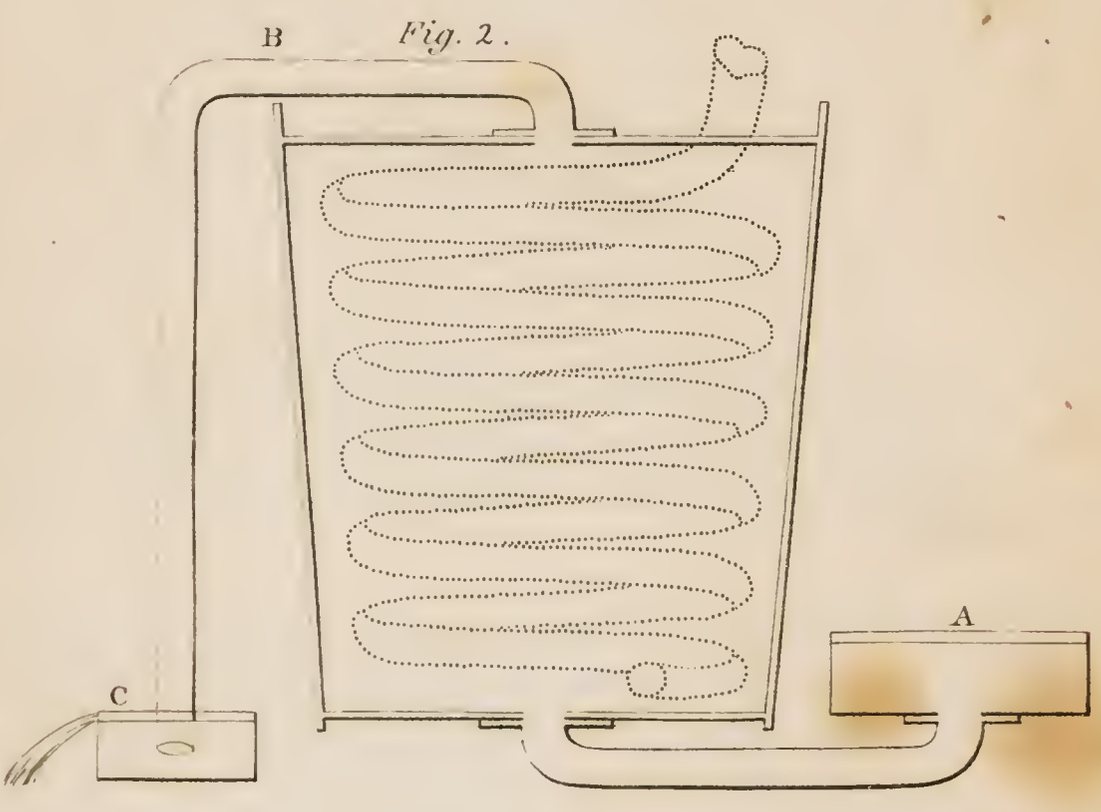
D.r Herschel on the magnified Image of the same Star, at different times.

Fig. 1.



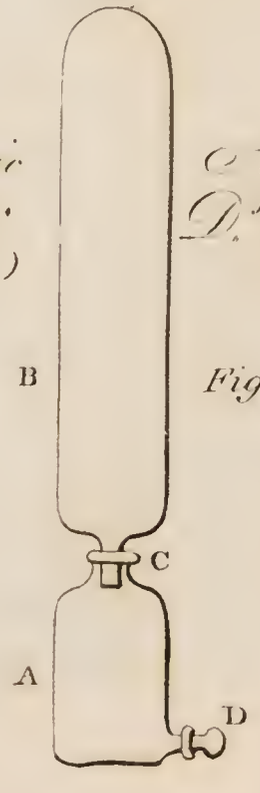
Sir A. N. Edelcrantz's method of easily raising Water in worm-tubs, condensers, &c.

Fig. 2.



Eudiometric Apparatus: By D.r Hope.

Fig. 3.



Mathew Sc. Rupsell Co.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1803.

ARTICLE I.

Analyſis of the Egyptian Heliotropium; a Mineral lately imported from that Country. By FREDERICK ACCUM, *Practical Chemiſt, and Teacher of Chemiſtry. Communicated by the Author.*

A PARTICULARLY beautiful ſpecies of ſilicious ſtone has lately been imported from Egypt, which was ſtated in the letter of the perſon who ſent it, to poſſeſs the peculiar property of reflecting the rays of the ſun red, when immerſed in water; and when taken out of this fluid to exhibit the figures of the ſun and of the moon, when viewed in a particular direction. But as none of the purchaſers of this mineral could make out theſe ſingular properties, the price fixed on the article was conſiderably diminiſhed, and the ſtone ſold at a cheap rate. The beautiful green colour which it poſſeſſes, and the capability of receiving a high poliſh, together with the facility of cutting it, has nevertheleſs rendered it a convenient article for being worked into different objects of fancy and ornament.

Beautiful ſiliceous ſtone of a green colour from Egypt.

The colour of this ſtone is a fine apple-green. It is very hard, and cannot be ſcratched, but with the point of a good pen-knife. Its fracture is even and free from all aſperities. It breaks with very ſharp edges, and its texture is very compact. It is ſemi-tranſparent when in pieces not exceeding half an

External appearance and properties.

inch in thickneſs. It ſtrikes fire with ſteel, and breaks into acute angular irregular pieces. Its ſpecific gravity is 2,708. The ſtone is ſold by the venders by the name of *Egyptian Heliotropium*. Its nature will be more clearly deduced from the following examination:

Analyſis.

Analyſis. Ignition rendered it of a chocolate brown, with white and yellow veins; the poliſh uninjured; loſs 1-21ſt part.

An entire poliſhed piece of the ſtone, weighing 250 grains, was expoſed to a white heat, in a wind-furnace, for two hours, and then ſuffered to cool. The original green colour of the mineral, was changed during this proceſs to a chocolate brown, with ſnow white and lemon yellow veins. The poliſh of the ſtone was not injured, but its weight was diminiſhed 12 grains.

Diffuſion of the powdered ſtone in four parts potaſh, fuſion, ſolution of the maſs in water, rather turbid.

Five hundred grains of the ignited ſtone were finely pulverized, and mixed with 2000 grains of potaſh, diſſolved in a like quantity of water. This mixture was evaporated to dryneſs in a crucible of platina, and expoſed to heat, gradually increaſed to whiteneſs, for one hour. During this operation the mixture fuſed quietly, and exhibited when cold, a homogeneous opake maſs, of a reddiſh colour. This maſs was covered with water, and after having been expoſed to a gentle heat, a ſolution of it was effected in that fluid.

Saturated with muriatic acid; gelatinous maſs. Dilution with water.

This alcaline ſolution was a little turbid, and could not be rendered tranſparent by repeated filtration. I ſaturated it with muriatic acid, a white precipitate fell down, the whole fluid acquired a reddiſh hue, and aſſumed a gelatinous conſiſtence. The latter could but difficultly be diminiſhed, by a copious admixture of water.

Evaporation nearly to dryneſs, addition of dilute muriatic acid; precipitate, *ſilex*.

In order to collect the precipitate which was diffuſed through the fluid, the whole was evaporated nearly to dryneſs, and then transferred into muriatic acid, diluted with ſix times its quantity of water, and afterwards filtered. The precipitate, after having been waſhed, dried, and ignited, weighed 365 grains. It was pure ſilex.

Reſidual fluid concentrated and ſaturated with carbonate of potaſh.

The fluid from which theſe earths had been ſeparated, together with the water expended for waſhing it, I concentrated by evaporation to about $\frac{1}{3}$ of its original bulk, and then ſaturated it with a heated ſolution of carbonate of potaſh in exceſs. The white precipitate which was depoſited, I transferred into a flaſk containing muriatic acid; a briſk effervescence enſued, but no perfect ſolution could be effected, by either this, or any other

other acid employed. It was therefore filtered, and the inſoluble part collected. This inſoluble reſidue was of a fine red colour, and harſh to the touch. It weighed $2\frac{1}{4}$ grains. But after having been boiled in muriatic acid, it loſt $\frac{1}{4}$ grain, and acquired a perfect white colour. The acid made uſe of for that purpoſe proved to have ſtripped it of iron to that amount. It was therefore a portion of ſilex which had eluded the firſt proceſs employed for ſeparating this earth.

Inſoluble (red) portion boiled with muriatic acid gave a little iron, and left ſilex.

The muriatic ſolution from which this ſiliceous earth had been ſeparated, I mingled with a ſolution of carbonate of potaſh, till no further turbidneſs enſued. The obtained precipitate was transferred after repeated ablutions in water, into a boiling ſolution of potaſh, and digeſted in that fluid for one hour. The mixture was then diluted with water, and the inſoluble part ſeparated by the filtre.

The laſt muriatic ſolution treated as before.

The alcaline ſolution was ſaturated with muriatic acid, and decompoſed by carbonate of ammonia in exceſs; the precipitate obtained by this means, after having acquired a conſiderable conſiſtence, was digeſted in acetous acid, and examined for magneſia, but no veſtige of ſuch earth could be detected. It was therefore dried and ignited, its weight amounted to $20\frac{1}{4}$ grains.

The alcaline ſolution was decompoſed by carbonate of ammonia, and digeſted in acetous acid, and the ſolution contained no magneſia, but only alumine.

The inſoluble reſidue from which this earth had been ſeparated, was again diſſolved in muriatic acid, and into this ſolution I dropped liquid ammonia till the odour of the latter conſiderably predominated. A brown flocculent precipitate fell down, which was collected, waſhed in liquid ammonia, and boiled for a few minutes in a ſolution of potaſh. It was then transferred into a ſmall retort, and nitric acid affuſed upon it, which was again diſtilled off from it repeatedly. The retort was laſtly heated to a dull redneſs. The precipitate now weighed 29 grains.

The reſidue not taken up by the acetous acid was again diſſolved in mur. acid, and precip. by exceſs of ammonia. This precip. after perfect ſeparation of all acid, and complete oxidation by nitric acid, was weighed as *oxide of iron*.

The fluid from which this precipitate had been obtained, together with the ſolution of potaſh made uſe of for boiling it, was then mixed with muriatic acid, and afterwards decompoſed by the addition of carbonate of potaſh. The product obtained, effervеſced with muriatic acid, and yielded ſulphate of lime on eſſaying a ſmall quantity of it by ſulphuric acid. It was therefore dried, and expoſed to a white heat, after which its weight was $56\frac{1}{2}$ grains.

The fluid from which the iron had been ſeparated was mixed with mur. acid and decompoſed by carb. potaſh. *Lime* fell down.

The reſiduary fluid was further examined in the uſual manner, but no other ſubſtance could be found, but what had been introduced during the different proceſſes made uſe of in this examination. The analyſis of the mineral being therefore compleated, from which it appears that 500 grains of the Egyptian heliotropium contain,

Component parts of Egyptian heliotropium.

Silex - - - - -	365 grains
Alumine - - - -	20 $\frac{1}{4}$
Oxide of Iron - - -	29 $\frac{1}{4}$
Lime - - - -	56 $\frac{1}{2}$
Water - - - -	24.
	495
Loſs	5
	500

Old Compton-Street, Soho,
Sept. 8th, 1803.

P. S. In my paper on the compound of phoſphorus and ſulphur in your laſt Journal (Auguſt) I obſerve a typographical error, p. 5. l. 22. which materially affects the ſenſe, and which I will thank you to correct, namely, for $\frac{1}{3}$, read $\frac{1}{38}$ part.

F. A.

II.

Method of cloſing wide mouthed Veſſels intended to be kept from communicating with the Air. In a Letter from ANTHONY CARLISLE, Eſq.

To Mr. NICHOLSON

DEAR SIR,

Cloſure of wide mouthed veſſels containing anatomical preparations.

IT is frequently deſirable to cloſe the openings of wide-mouthed veſſels intended to contain ſubſtances which would be injured by free expoſure to the atmoſphere, or to evaporation. The preſent obſervations, however, originate in attempts made to improve the art of preſerving anatomical preparations.—The moſt uſual liquids employed for what are termed "wet preparations," are weak ardent ſpirits, and diſtilled oil of turpentine,

pentine, to which may be added an aqueous ſolution of hydrargyrus muriatus, or mercury corroſive ſublimate in the proportion of twenty grains of the latter to a pound of pure water.

Uſual methods.

The methods ordinarily adopted for cloſing glaſs veſſels uſed for theſe purpoſes, have been ground glaſs ſtoppers, well ſoaked bladders with a middle plate of thick ſheet lead, to keep the top level, and plates of glaſs luted with glaziers putty.

Objections to each ſeverally.

The objections to theſe methods are found by experience to be ſufficient to induce the trial of others. The ground glaſs ſtoppers are ſeldom air-tight, but when they are, it happens that by the accumulating of particles of duſt in the fitting, the ſtopper becomes in a few years immoveable. Where oil of turpentine is employed, the ſtopper becomes fixed by hardened turpentine. The ſtopping with bladders and ſheet lead is liable to ſuch repeated changes of preſſure within and without, by the alterations in the expanſion of the contained liquids from variations of temperature; that the coheſion of the bladders are eventually deſtroyed; add to this, that ſuch preparations cannot be taken out of the veſſels for examination, or the liquor renewed, without the trouble of a new ſtopping. The plate of glaſs with putty is ſeldom air tight, but when it is ſo, the ſtopping is liable to the ſame objection which was ſtated laſtly againſt the bladders.

New method by a jar, the cover of which fits into a groove with hog's lard.

The method I have now adopted, is to have a glaſs jar with a groove half an inch deep round the outſide of the top or mouth, and a glaſs lid, like that uſed by confectioners in their ſhow glaſſes, the lid fitting looſely into the groove is rendered air tight by hog's lard, a ſubſtance never quite fluid at the higheſt temperature of this climate, and always ſoft enough in the cold ſeaſon to admit of removing the lid or top.

The firſt glaſs of this kind was made to my order by Mr. Parker in Fleet-ſtreet, to contain twenty ounce meaſures of water, and the coſt was five ſhillings. A ſimilar adjuſtment for the lids of earthen jars, to contain pickles, preſerves, &c. ſeems both eligible and eaſy in practice. See Plate V. Fig. 2.

I am,

DEAR SIR,

Your obliged Friend,

A. CARLISLE.

Soho-Square.

Extract

III.

*Extract of a Letter from Toulon to General le Vavaſſeur, Inſpector of the Materials of the Guns of the French Navy, on the Changes which Caſt Iron undergoes by remaining long in the Sea.**

Caſt iron lying thirty years at the bottom of the ſea was oxided, Not uniformly, but in veins.

A cannon ſunk in a ſhip burned at the evacuation of Toulon was oxided only in the middle.

AN obſervation I have never heard explained is, that caſt iron, which has lain a long time at the bottom of the ſea, is not equally oxided throughout. I formerly ſaw a cannon weighed up, after it had been ſunk thirty years, which was ſo much oxided in veins, that I could run a knife into ſome places, while the metal cloſe by was impenetrable; and on carrying the knife beyond this hard vein, it entered as before. A gun has juſt been weighed up here (at Toulon,) belonging to one of the ſhips burnt when the Engliſh evacuated the city. The middle is ſo uniformly oxided, that a large piece may be cut off with a hatchet. Toward the breech, and toward the mouth, the metal appears to have loſt nothing of its hardneſs. Can this difference be aſcribed to the contact of the ſubſtances in which the gun was buried underneath the water? Its poſition at the bottom of the ſea not being known, we can form no conjectures on this point. For my part I had imagined, from the hard veins of the cannon mentioned above, that its metal had intermixed with it ſubſtances on which ſalt water could not act. The gun lately taken out of the ſea appeared more homogenous, but I cannot frame any ſatisfactory explanation of the fact.†

* *Annales de Chimie*, V. 130.

† It is probable, that the laſt cannon, as it belonged to a ſhip that was burned, had part of it heated to ſuch a point, when it fell into the ſea, as would occaſion it to be oxided in a higher degree than the others. It appears to me, more difficult to explain the different veins exhibited by the former cannon. *Note of General le Vavaſſeur.*

IV.

On the Antiquity of the Invention of Gun-powder, and its firſt Application to Military Purpoſes. By Mr. WIEGLEB.*

THE period of the invention of Gun-powder, and its firſt application for the purpoſe of artillery, has not yet been accurately aſcertained. Though there are many accounts which have been given concerning the invention of this deſtructive compound, yet none of them ſtate their authorities. The uncertainty concerning the invention of gun-powder ſeems merely to be owing to the want of proper documents. The moſt ſummary accounts the author of this paper could find, are contained in two Eſſays, the one written by Gram, the other by Tremler. The firſt account contains the hiſtory of the invention of gun-powder in Europe, and its firſt application in Denmark; the latter comprehends both the invention of powder, and the uſe of guns among the Europeans.

Hiſtorical facts to determine the firſt invention of gunpowder, and its uſe in war.

According to the opinion of Gram, gun-powder was already known in Europe about the year 1340. Tremler, on the contrary, endeavours to prove, that no author of credit has poſitively ſhewn that it was known before the year 1354. Theſe authors, therefore, differ only 14 years, reſpecting the period of the invention. Being the other day, ſays Mr. Wiegleb, in ſearch of ſome documents depoſited in the archives of this town (Langenſalze), I happened to meet with the annual account of the expences of the town for the year 1378. This account contains a ſpecification of different arms, viz. pikes, bows, croſs-bows, arrows, guns and gun-powder. The following articles particularly fixed his attention:

1. One gun. 2. One gun and one charge of lead. 3. One gun and one charge of lead. 4. Two guns and two charges of lead. 5. One gun and one charge of lead. 6. Two guns and three charges of lead. Beſides theſe articles, the following charge was made in the account:

Pro Pulveribus, 35 ſhillings.

Pro Pulveribus quos domini emerunt ad Pyxides, 3 ſhillings.

Pro duabus Patellis ad Pyxides, 3 ſhillings.

* From the German of Crell's Annales, v. xx. p. 6.

If

Hiſtorical facts to determine the firſt invention of gunpowder, and its uſe in war.

If we conſider that guns, powder, and lead, are here charged, it is obvious that by the word *pulvis*, gun-powder muſt be underſtood; and by *patellis*, guns muſt be meant. From theſe documents we are led to believe that guns and gun-powder were known already before the year 1378. And it is more than probable that they were not purchaſed that year, but had probably been uſed before that time. For guns were too expenſive for ſingle individuals and ſmall towns at that time; and on that account, the place in which I met with the ſaid documents was very probably provided with guns many years after the invention of them. That this muſt have been the caſe becomes obvious from the following obſervation: Achilles Geſner, the Hiſtorian of Augſburg, who wrote a Latin Chronicle at the beginning of the 16th century, ſays,* "Three large cannons were caſt at Augſburg in the "year 1378, the largeſt of which diſcharged a ball of 127 "pounds; the ſecond a ball of 70 pounds, and the third a "ball of 50 pounds, at a diſtance of 1000 paces."

Herman Corner, who lived at the end of the 14th century, relates that the inhabitants of Lubec aſſiſted the Emperor Charles, who beſieged the caſtle of Dannenberg, with 600 *armatis cum duabus machinis. Bombardae enim pro tunc non erant ita communes, uti nunc ſunt:* From hence it is evident that the 600 machines were nothing but bombardae, or guns.

Another remarkable document the author of this paper met with was the ſentence of death of Nicolaus the Bold, who ſupplied the enemy with two barrels of gun-powder, in the year 1372. In this ſentence it is clearly expreſſed, that the gun-powder was made up of ſaltpetre and ſulphur. In the ſame year, the Corporation of Augſburg ordered to have caſt, twenty cannons of metal, at the great expence of fifty pence. Theſe cannons were intended to be uſed againſt their neighbours, the Bavarians.

Petrarch, born 1304, ſtates, in his work † publiſhed 1374.

GAV. *Habeo machinas, ingentia ſaxa torquentes.*

RAL. *Saxa torquere furioſum eſt.*

GAV. *Habeo machinas et balliſtas innumeras.*

RAL. *Mirum niſi et glandes Aeneas quae flammis ejectis horriſon tone tru jacuntur. Non erat ſates de coelo tonantes ira*

* Annales Augſburgenſes.

† De Remediis utriuſque fortunae.

Hiſtorical facts to determine the firſt invention of gunpowder, and its uſe in war.

Dei immortalis, homuncio? O credulitas juncta ſuberbiae. De terra etiam tonuiſſet: Non immutabile fulmen ut Maro ait humana rabies immitata eſt, et quod e nubibus mitti ſolet, ligneo quidem, ſed tartareo mittitur inſtrumento. Erat haec peſtis nuper rara ut cum ingenti miraculo cerneretur, nunc ut rerum peſſimarum dociles ſunt animi, ita communis eſt ut unum quodlibet genus armorum.

We ſhall be leſs ſurpriſed that cannons and guns were made of wood; even in the 15th century guns were bound with iron hoops.

In the year 1365, Margraff Frederick, of Meiſſen, attempted to ſtorm the town and caſtle of Einben with ſlings, battering rams, and other machines, then made uſe of in beſieging towns. Rothe, who mentions this in his Chronicle of Thuringen, farther relates that the Duke Albert was in poſſeſſion of a gun which he himſelf uſed at the ſiege, for ſhooting into the works of the enemy. It was, ſay this author, the firſt gun ever ſeen in that country.

In another document is ſtated, that *anno Domini, milleſimo tricenteſimo ſexageſimo, conſiſtorium urbis Lubecenſis in toto combuſtum eſt, per negligentiem illorum qui pulveras pro bombardis parabant.* The ſame fire is mentioned by Herman Corner, a native of Lubeck. His words, as taken from the Chronicle of Lubeck, are as follows: *Conſiſtorium urbis Lubecenſis incenſum eſt, et combuſtum per negligentiam illorum qui pulveres pro bombarbis, ſive petraries parabant, ſecundum Chronicam Lubecenſem. Cum enim praedictas paraſſent locabant eos in quodam loco conſiſtoris non caute cuſtoditos ab igne. Pulveres ergo per incuriam nocte accenſi, domum ipſam ſuccenderunt, ad antequam extingui potuiſſent, eum in cineres redegerunt.* Conſequently gunpowder muſt have been prepared already at Lubeck about the year 1360.

In the year 1359, a war broke out between the kings of Caſtile and Arragonia; in which the latter made uſe of a large gun, with which he did much damage to the veſſels of the king of Caſtile; for he ſhot down with it the maſts and rigging, and killed many men by only two ſhots.

Peter Divacus commemorates, that in the year 1356 the inhabitants of Lyons in Brabant purchaſed 12 guns (bombardae) which were called thundering guns, or blunderbuſſes, *ab horrendo fragore.*

V.

A Chemical Analyſis of ſome Calamines. *By* JAMES SMITHSON, Esq. F. R. S. P. R. S. *From the Philoſophical Tranſactions for* 1803.

Reſearches of Bergman on Calamines.

NOTWITHSTANDING the experiments of Bergman and others, on thoſe ores of zinc which are called calamine, much uncertainty ſtill ſubſiſted on the ſubject of them. Their conſtitution was far from decided, nor was it even determined whether all calamines were of the ſame ſpecies, or whether there were ſeveral kinds of them.

Hauy's opinon that they are pure oxides.

The Abbé Hauy, ſo juſtly celebrated for his great knowledge in cryſtallography and mineralogy, has adhered, in his late work *, to the opinions he had before advanced †, that calamines were all of one ſpecies, and contained no carbonic acid, being a ſimple calx of zinc, attributing the effervescence which he found ſome of them to produce with acids, to an accidental admixture of carbonate of lime.

Experiments.

The following experiments were made to obtain a more certain knowledge of theſe ores; and their reſults will ſhow the neceſſity there was for their farther inveſtigation, and how wide from the truth have been the opinions adopted concerning them.

Calamine from Bleyberg.

Calamine from Bleyberg.

a. The ſpecimen which furniſhed the ſubject of this article, was ſaid by the German of whom it was purchaſed, to have come from the mines of Bleyberg in Carinthia.

External characters.

It was in the form of a ſheet ſtalactite, ſpread over ſmall fragments of limeſtone. It was not however at all cryſtalline, but of the dull earthy appearance of chalk, though, on compariſon, of a finer grain and cloſer texture.

It was quite white, perfectly opaque, and adhered to the tongue; 68.0 grs. of it, in ſmall bits, immerſed in diſtilled water, abſorbed 19.8 grs. of it, = 0,29.

It admitted of being ſcraped by the nail, though with ſome difficulty: ſcraped with a knife, it afforded no light.

* *Traité de Mineralogie*, Tome IV. † *Journal des Mines.* No. 32.

68.1 grs.

68.1 grs. of it, broken into ſmall pieces, expelled 19.0 grs. of diſtilled water from a ſtopple bottle. Hence its denſity = 3.584. In another trial, 18.96 grs. at a heat of 65° Fahrenheit, diſplaced 5.27 grs. of diſtilled water; hence the denſity = 3.598. The bits, in both caſes, were entirely penetrated with water. Specific gravity.

b. Subjected to the action of the blowpipe on the coal, it became yellow the moment it was heated, but recovered its priſtine whiteneſs on being let cool. This quality, of temporarily changing their colour by heat, is common to moſt, if not all, metallic oxides; the white growing yellow, the yellow red, the red black. Blowpipe aſſays.

Urged with the blue flame, it became extremely friable; ſpread yellow flowers on the coal; and, on continuing the fire no very long time, entirely exhaled. If the flame was directed againſt the flowers, which had ſettled on the coal, they ſhone with a vivid light. A bit fixed to the end of a ſlip of glaſs, waſted nearly as quickly as on the coal.

It diſſolved in borax and microcoſmic ſalt, with a ſlight efferveſcence, and yielded clear colourleſs glaſſes; but which became opaque on cooling, if over ſaturated. Carbonate of ſoda had not any action on it.

c. 68.0 grs. of this calamine diſſolved in dilute vitriolic acid with a briſk efferveſcence, and emitted 9.2 grs. of carbonic acid. The ſolution was white and turbid, and on ſtanding depoſited a white powder, which, collected on a ſmall filter of gauze paper, and well edulcorated and let dry, weighed only 0.86 grs. This ſediment, tried at the blowpipe, melted firſt into an opaque white matter, and then partially reduced into lead. It was therefore, probably, a mixture of vitriol of lead and vitriol of lime. Solutions in the ſulphuric and muriatic acids gave only ſalts of zinc, and gave carbonic acid.

The filtered ſolution, gently exhaled to dryneſs, and kept over a ſpirit-lamp till the water of cryſtallization of the ſalt and all ſuperfluous vitriolic acid were driven off, afforded 96.7 grs. of perfectly dry, or *arid* *, white ſalt. On re-ſolution in water, and cryſtallization, this ſaline matter proved to be wholly vitriol

* *Dry*, as oppoſed to wet or damp, which are only degrees of each other, merely implies free from mechanically admixed water. *Arid*, may be appropriated to expreſs the ſtate of being devoid of combined water.

of

of zinc, excepting an inappretiable quantity of vitriol of lime in capillary cryſtals, due, without doubt, to a ſlight and accidental admixture of ſome portion of the calcareous fragments on which this calamine had been depoſited. Pure martial pruſſiate of tartar, threw down a white precipitate from the ſolution of this ſalt.

In another experiment, 20.0 grs. of this calamine afforded 28.7 grs of arid vitriol of zinc.

d. 10 grs. of this calamine were diſſolved in pure marine acid, with heat. On cooling, ſmall capillary cryſtals of muriate of lead formed in the ſolntion. This ſolution was precipitated by carbonate of ſoda, and the filtered liquor let exhale ſlowly in the air; but it furniſhed only cryſtals of muriate of ſoda.

e. 10 grs. diſſolved in acetous acid without leaving any reſiduum. By gentle evaporation, 20.3 grs. = 2.03. of acetite of zinc, in the uſual hexagonal plates, were obtained These cryſtals were permanent in the air, and no other kind of ſalt could be perceived amongſt them.

Neither ſolution of vitriolated tartar, nor vitriolic acid, occaſioned the ſlighteſt turbidneſs in the ſolution of theſe cryſtals, either immediately or on ſtanding; a proof that the quantity of lime and lead in this ſolution, if any, was exceſſively minute.

f. A bit of this calamine, weighing 20.6 grs. being made red hot in a covered tobacco-pipe, became very brittle, dividing on the ſlighteſt touch into priſms, like thoſe of ſtarch, and loſt 5.9 grs. of its weight = 0.286. After this, it diſſolved ſlowly and difficultly in vitriolic acid, without any efferveſcence.

The calamine contained oxide of zinc, carbonic acid, and water.

According to theſe experiments, this calamine conſiſts of,

Calx of zinc - - -	0.714
Carbonic acid - - -	0.135
Water - - - - -	0.151
	1.000.

The carbonates of lime and lead in it are more accidental admixtures, and in too ſmall quantity to deſerve notice.

Calamine from Somerſetſhire.

Somerſetſhire calamine.

a. This calamine came from Mendip Hills in Somerſetſhire. It had a mammillated form; was of a denſe cryſtalline texture; ſemitranſparent at its edges, and in its ſmall fragments; and upon the whole very ſimilar, in its general appearance, to calcedony.

It

It was tinged, exteriorly, brown; but its interior colour was a greeniſh yellow.

It had conſiderable hardneſs; it admitted however of being ſcraped by a knife to a white powder. Characters.

56.8 grs. of it diſplaced 13.1 grs. of water, at a temperature of 65_{o} Fahrenheit. Hence its denſity = 4.336.

b. Expoſed to the blowpipe, it became opaque, more yellow, and friable; ſpread flowers on the coal, and conſequently volatilized, but not with the rapidity of the foregoing kind from Bleyberg. Blowpipe aſſays.

It diſſolved in borax and microcoſmic ſalt, with efferveſcence, yielding colourleſs glaſſes. Carbonate of ſoda had no action on it.

c. It diſſolved in vitriolic acid with a briſk efferveſcence; and 67.9 grs. of it emitted 24.5 grs. = 0.360, of carbonic acid. This ſolution was colourleſs; and no reſiduum was left. By evaporation, it afforded only vitriol of zinc, in pure limpid cryſtals. Solution.

d. 23.0 grs. in ſmall bits, made red hot in a covered tobacco-pipe, loſt 8.1 grs. = 0.352. It then diſſolved ſlowly and difficultly in vitriolic acid, without any emiſſion of carbonic acid; and, on gently exhaling the ſolution, and heating the ſalt obtained, till the expulſion of all ſuperabundant vitriolic acid and all water, 29.8 grs. of arid vitriol of zinc were obtained. This dry ſalt was wholly ſoluble again in water; and ſolution of pure martial pruſſiate of ſoda occaſioned a white precipitate in it.

This calamine hence conſiſts of, Component parts, Carbonic acid and oxide of zinc.

Carbonic acid - - -	0.352
Calx of zinc - - - -	0.648
	1.000.

Calamine from Derbyſhire.

a. This calamine conſiſted of a number of ſmall cryſtals, about the ſize of tobacco-ſeeds, of a pale yellow colour, which appeared from the ſhape of the maſs of them, to have been depoſited on the ſurface of cryſtals of carbonate of lime, of the form of Fig. 28. Plate IV. of the *Criſtallographie* of Romé de L'Iſle. Derbyſhire calamine.

The ſmallneſs of theſe calamine cryſtals, and a want of ſharpneſs rendered it impoſſible to determine their form with certainty: External characters, &c.

certainty; they were evidently, however, rhomboids, whoſe faces were very nearly, if not quite, rectangular, and which were incomplete along their ſix intermediate edges, apparently like Fig. 78. Plate IV. of Romé de L'Iſle.

22.1 grs. of theſe cryſtals, at a heat of 57° Fahrenheit, diſplaced 5.1 grs. of water, which gives their denſity = 4.333.

Heat did not excite any electricity in theſe cryſtals.

Experiments.

b. Before the blowpipe, they grew more yellow and opaque, and ſpread flowers on the coal. They diſſolved wholly in borax and microcoſmic ſalt, with efferveſcence.

c. 22.0 grs. during their ſolution in vitriolic acid, efferveſced, and loſt 7.8 grs. of carbonic acid = 0.354. This ſolution was colourleſs, and afforded 26.8 grs. of arid vitriol of zinc, which, rediſſolved in water, ſhot wholly into clear colourleſs priſms of this ſalt.

Component parts carbonic acid and oxide of zinc.

d. 9.2 grs. of theſe cryſtals, ignited in a covered tobacco-pipe, loſt 3.2 grs. = 0.3478; hence theſe cryſtals conſiſt of,

Carbonic acid - - -	0.348
Calx of zinc - - - -	0.652
	1.000.

Electrical Calamine.

Electrical calamine from Regbania.

The Abbé Hauy has conſidered this kind as differing from the other calamines only in the circumſtance of being in diſtinct cryſtals; but it has already appeared, in the inſtance of the Derbyſhire calamine, that all cryſtals of calamine are not electric by heat, and hence, that it is not merely to being in this ſtate that this ſpecies owes the above quality. And the following experiments, on ſome cryſtals of electric calamine from Regbania in Hungary, can leave no doubt of its being a combination of calx of zinc with quartz; ſince the quantity of quartz obtained, and the perfect regularity and tranſparency of theſe cryſtals make it impoſſible to ſuppoſe it a foreign admixture in them.

a. 23.45 grs. of theſe Regbania cayſtals, diſplaced 6.8 grs. of diſtilled water, from a ſtopple-bottle, at the temperature of 64° Fahrenheit; their ſpecific gravity is therefore = 3.434.

The form of theſe cryſtals is repreſented in Figure I. Plate V. where the angle formed by the planes *a* and *c* was 90°, that by *a* and *e* = 150°, that by *b* and *c* = 115°, and that by *c* and *d* = 130°.

They were not ſcratched by a pin; a knife marked them.

b. One of theſe cryſtals, expoſed to the flame of the blow-pipe, decrepitated and became opaque, and ſhone with a green light, but ſeemed totally infuſible. Blowpipe experiments.

Borax and microcoſmic ſalt diſſolved theſe cryſtals, without any efferveſcence, producing clear colourleſs glaſſes, Carbonate of ſoda had little if any action on them.

c. According to Mr. Pelletier's experiments * on the calamine of Fribourg in Briſgaw, which is undoubtedly of this ſpecies, its compoſition is,

Quartz - - - - -	0.50
Calx of zinc - - - -	0.38
Water - - - - -	0.12
	1.00.

The experiments on the Regbania cryſtals have had different reſults; but, though made on much ſmaller quantities, they will perhaps not be found, on repetition, leſs in conformity with nature.

23.45 grs. heated red hot in a covered crucible, decrepitated a little, and became opaque, and loſt 1.05 grs. but did not fall to powder or grow friable. It was found, that this matter was not in the leaſt deprived of its electrical quality by being ignited; aad hence, while hot, the fragments of theſe decrepitated cryſtals clung together, and to the crucible.

d. 22.2 grs. of theſe decripitated cryſtals, = 23.24 grs. of the original cryſtals, in a ſtate of impalpable powder, being digeſted over a ſpirit-lamp with diluted vitriolic acid, ſhowed no efferveſcence; and, after ſome time, the mixture became a jelly. Exhaled to dryneſs, and ignited ſlightly, to expel the ſuperfluous vitriolic acid, the maſs weighed 37.5 grs.

On extraction of the ſaline part by diſtilled water, a fine powder remained, which, after ignition, weighed 5.8 grs. and was quartz.

The ſaline ſolution afforded, on cryſtallization, only vitriol of zinc. Theſe cryſtals therefore conſiſt of, Component parts of electrical calamine.

Quartz - - - -	0.250
Calx of zinc - - - -	0.683
Water - - - - -	0.044
	0.977
Loſs - - - - -	0.023
	1.000.

* Journal de Phyſique. Tom. XX. p. 414.

The

The water is moſt probably not an eſſential element of this calamine, or in it in the ſtate of, what is improperly called, water of cryſtallization, but rather exiſts in the cryſtals in fluid drops interpoſed between their plates, as it often is in cryſtals of nitre, of quartz, &c. Its ſmall quantity, and the cryſtals not falling to powder on its expulſion, but retaining almoſt perfectly their original ſolidity, and ſpathoſe appearance in the places of fracture, and, above all, preſerving their electrical quality wholly unimpaired, which would hardly be the caſe after the loſs of a real element of their conſtitution, ſeem to warrant this opinion.

If the water is only accidental in this calamine, its compoſition, from the above experiments, will be,

Quartz - - - - -	0.261
Calx of zinc - - -	0.739
	1.000

It is found in Derbyſhire.

I have found this ſpecies of calamine amongſt the productions of Derbyſhire, in ſmall brown cryſtals, depoſited, together with the foregoing ſmall cryſtals of carbonate of zinc, on cryſtals of carbonate of lime. Their form ſeems, as far as their minuteneſs and compreſſion together would allow of judging, nearly or quite the ſame as that of thoſe from Regbania; and the leaſt atom of them immediately evinces its nature, on being heated, by the ſtrong electricity it acquires. On their ſolution in acids, they leave quartz.

OBSERVATIONS.

Chemiſtry is yet ſo new a ſcience, what we know of it bears ſo ſmall a proportion to what we are ignorant of, our knowledge in every department of it is ſo incomplete, ſo broken, conſiſting ſo entirely of iſolated points thinly ſcattered like lucid ſpecks on a vaſt field of darkneſs, that no reſearches can be undertaken without producing ſome facts, leading to ſome conſequences, which extend beyond the boundaries of their immediate object.

Component parts of ſulphate of zinc.

1. The foregoing experiments throw light on the proportions in which its elements exiſt in vitriol of zinc. 23.0 grs. of the Mendip Hill calamine, produced 29.8 grs. of arid vitriol of zinc. Theſe 23.0 grs. of calamine contained 14.9 grs. of calx

calk of zinc; hence, this metallic ſalt, in an arid ſtate, conſiſts of *exactly equal* parts of calx of zinc and vitriolic acid.

This inference is corroborated by the reſults of the other experiments: 68.0 grs. of the Bleyberg calamine, containing 48.6 grs. of calx of zinc, yielded 96.7 grs. of arid vitriol of zinc; and, in another trial, 20.0 grs. of this ore, containing 14.2 grs. of calx of zinc, produced 28.7 grs. of arid vitriol of zinc. The mean of theſe two caſes, is 62.7 grs. of arid vitriol of zinc, from 31.4 grs. of calx of zinc.

In the experiment with the cryſtals of carbonate of zinc from Derbyſhire, 14.35 grs. of calx of zinc furniſhed indeed only 26.8 grs of arid vitriol of zinc; a deficiency of about $\frac{6}{100}$, occaſioned probably by ſome ſmall inaccuracy of manipulation.

Poſition that the parts of compounds do not greatly exceed each other in quantity.

2. When the ſimplicity found in all thoſe parts of nature which are ſufficiently known to diſcover it is conſidered, it appears improbable that the proximate conſtituent parts of bodies ſhould be united in them, in the very remote relations to each other in which analyſes generally indicate them; and, an attention to the ſubject has led me to the opinion that ſuch is in fact not the caſe, but that, on the contrary, they are univerſally, as appears here with reſpect to arid vitriol of zinc, fractions of the compound of very low denominators. Poſſibly in few caſes exceeding five.

The ſucceſs which has appeared to attend ſome attempts to apply this theory, and amongſt others, to the compoſitions of ſome of the ſubſtances above analyſed, and eſpecially to the calamine from Bleyberg, induces me to venture to dwell here a little on this ſubject, and ſtate the compoſition of this calamine, which reſults from the ſyſtem, as, beſides contributing perhaps to throw ſome light on the true nature of this ore, it may be the means likewiſe of preſenting the theory under circumſtances of agreement with experiment, which, from the ſurpriſing degree of nearneſs, and the trying complexity of the caſe, may ſeem to entitle it to ſome attention.

Hence the component parts of Bleyberg calamine are ſuppoſed to be arranged in ſubordinate compounds.

From this calamine, containing, according to the reſults of the experiments on the Mendip Hill kind, too ſmall a quantity of carbonic acid to ſaturate the whole of the calx of zinc in it, and from its containing much too large a portion of water to be in it in the ſtate of mere moiſture or dampneſs, it ſeems to conſiſt of two matters; carbonic of zinc, and a peculiar compound of zinc and water, which may be named *hydrate of zinc*.

By the results of the analysis of the Mendip-Hill calamine, corrected by the theory, carbonate of zinc appears to consist of,

Carbonic acid - - - - -	$\frac{1}{3}$
Calx of zinc - - - - -	$\frac{2}{3}$

Deducting from the calx of zinc in the Bleyberg calamine, that portion which corresponds, on these principles, to its yield of carbonic acid, the remaining quantity of calx of zinc and water are in such proportions as to lead, from the theory, to consider hydrate of zinc as composed of

Calx of zinc - - - - -	$\frac{3}{4}$
Water, or rather ice - - - -	$\frac{1}{4}$

And, from these results, corrected by the theory, I consider Bleyberg calamine as consisting of,

Carbonic of zinc - - - -	$\frac{2}{5}$
Hydrate of zinc - - - -	$\frac{3}{5}$

The test of this hypothesis is in the quantities of the remote elements which analysis would obtain from a calamine thus composed.

The following table will show how very insignificantly the calamine compounded by the theory, would differ in this respect from the calamine of nature.

Elucidations of chemical theory.

1000 parts of the compound salt of carbonate and hydrate of zinc consist of,

Carbonate of zinc 400 - - - - =	Carbonic acid	$= \frac{400}{3} =$ - - - - -	$133\frac{1}{3}$
	Calx of zinc	$= \frac{400 \times 2}{3} = 266\frac{2}{3}$	$= 716\frac{2}{3}$
Hydrate of zinc = 600 - - - - =	Calx of zinc	$= \frac{600 \times 3}{4} = 450$	
	Ice - - -	$= \frac{600}{4} =$ - - - - - -	150
			1000.

Great as is the agreement between the quantities of the last column and those obtained by the analysis of the Bleyberg calamine, it would be yet more perfect, probably, had there been, in this instance, no sources of fallacy but those attached to chemical operations, such as errors of weighing, waste, &c. but

but the differences which exiſt are owing, in ſome meaſure at leaſt, to the admixture of carbonate of lime and carbonate of lead, in the calamine analyſed, and alſo to ſome portion of water, which is undoubtedly contained, in the ſtate of moiſture, in ſo porus and bibulous a body. Elucidations of chemical theory.

It has alſo appeared, in the experiments on the Mendip Hill calamine, that acids indicate a greater quantity of carbonic acid than fire does, $\frac{22}{1000}$. If we make this deduction for diſſolved water, it reduces the quantity of carbonic acid in the Bleyberg calamine, to 0.1321.

If we aſſume this quantity of carbonic acid as the datum to calculate, on this ſyſtem, the compoſition of the calamine from Bleyberg, we ſhall obtain the following reſults:

Compound ſalt, of carbonate of zinc and hydrate of zinc	990.3
Water in the ſtate of moiſture - - - -	2.5
Carbonate of lime and carbonate of lead - -	7.2
	1000.0

It may be thought ſome corroboration of the ſyſtem here offered, that, if we admit the proportions which it indicates, the remote elements of this ore, while they are regular parts of their immediate products, by whoſe ſubſequent union this ore is engendered, are alſo regular fractions of the ore itſelf: thus,

The carbonic acid - - -	$=\frac{8}{60}$
The water - - - -	$=\frac{9}{60}$
The calx of zinc - - - -	$=\frac{43}{60}$

Hereby diſplaying that ſort of regularity, in every point of view of the object, which ſo wonderfully characteriſes the works of nature, when beheld in their true light.

If this calamine does conſiſt of carbonate of zinc and hydrate of zinc, in the regular proportions above ſuppoſed, little doubt can exiſt of its being a true chemical combination of theſe two matters, and not merely a mechanical mixture of them in a pulverulent ſtate; and, if ſo, we may indulge the hope of ſome day meeting with this ore in regular cryſtals.

If the theory here advanced has any foundation in truth, the diſcovery will introduce a degree of rigorous accuracy and certainty into chemiſtry, of which this ſcience was thought to be ever incapable, by enabling the chemiſt, like the geometrician, to rectify by calculation the unavoidable errors of his

Elucidations of chemical theory.

manual operations, and by authoriſing him to eliminate from the eſſential elements of a compound, thoſe products of its analyſis whoſe quantity cannot be reduced to any admiſſible proportion.

A certain knowledge of the exact proportions of the conſtituent principles of bodies, may likewiſe open to our view harmonious analogies between the conſtitutions of related objects, general laws, &c. which at preſent totally eſcape us. In ſhort, if it is founded in truth, its enabling the application of mathematics to chemiſtry, cannot but be productive of material reſults *.

3. By the application of the foregoing theory to the experiments on the electrical calamine, its elements will appear to be,

Quartz - - - - -	$\frac{1}{4}$
Calx of zinc - - - - -	$\frac{3}{4}$

A ſmall quantity of the calamine having eſcaped the action of the vitriolic acid, and remained undecompoſed, will account for the ſlight exceſs in the weight of the quartz.

4. The exhalation of theſe calamines at the blowpipe, and the flowers which they diffuſe round them on the coal, are probably not to be attributed to a direct volatilization of them. It is more probable that they are the conſequences of the diſoxidation of the zinc calx, by the coal and the inflammable matter of the flame, its ſublimation in a metallic ſtate, and inſtantaneous recalcination. And this alternate reduction and combuſtion, may explain the peculiar phoſphoric appearance exhibited by calces of zinc at the blowpipe.

The apparent ſublimation of the common flowers of zinc at the inſtant of their production, though totally unſublimable afterwards, is certainly likewiſe but a deceptious appearance. The reguline zinc, vaporiſed by the heat, riſes from the crucible as a metallic gas, and is, while in this ſtate, converted to a calx. The flame which attends the proceſs is a proof of it; for flame is a maſs of vapour, ignited by the production of fire within itſelf. The fibrous form of the flowers of zinc, is owing to a cryſtallization of the calx while in *mechanical ſuſ-*

* It may be proper to ſay, that the experiments have been ſtated *preciſely* as they turned out, and have not been in the *leaſt degree* bent to the ſyſtem.

penſion

penſion in the air like that which takes place with camphor when, after having been ſome time inflamed, it is blown out. Elucidations of chemical theory.

A moment's reflection muſt evince, how injudicious is the common opinion, of cryſtallization requiring a ſtate of ſolution in the matter; ſince it muſt be evident, that while ſolution ſubſiſts, as long as a quantity of fluid admitting of it is preſent, no cryſtallization can take place. The only requiſite for this operation, is a freedom of motion in the maſſes which tend to unite, which allows them to yield to the impulſe which propels them together, and to obey that ſort of polarity which occaſions them to preſent to each other the parts adapted to mutual union. No ſtate ſo completely affords theſe conditions as that of mechanical ſuſpenſion in a fluid whoſe denſity is ſo great, relatively to their ſize, as to oppoſe ſuch reſiſtance to their deſcent in it as to occaſion their mutual attraction to become a power ſuperior to their force of gravitation. It is in theſe circumſtances that the atoms of matters find themſelves, when, on the ſeparation from them of the portion of fluid by which they were diſſolved, they are abandoned in a diſengaged ſtate in the boſom of a ſolution; and hence it is in ſaturated ſolutions ſuſtaining evaporation, or equivalent cooling, and free from any perturbing motion, that regular cryſtallization is uſually effected.

But thoſe who are familiar with chemical operations, know the ſort of agglutination which happens between the particles of ſubſided very fine precipitates: occaſioning them, on a ſecond diffuſion through the fluid, to ſettle again much more quickly than before, and which is certainly a cryſtallization, but under circumſtances very unfavourable to its perfect performance.

5. No calamine has yet occurred to me which was a real, uncombined, calx of zinc. If ſuch, as a native product, ſhould ever be met with in any of the ſtill unexplored parts of the earth, or exiſt amongſt the unſcrutinized poſſeſſions of any cabinet, it will eaſily be known, by producing a quantity of arid vitriol of zinc exactly double its own weight; while the hydrate of zinc, ſhould it be found ſingle, or uncombined with the carbonate, will yield, it is evident, 1.5 its weight of this arid ſalt.

TABLE

VI.

Table of the Radii of Wheels, from Ten to Three Hundred Teeth, the Pitch being Two Inches. By Mr.* B. Donkin, *Millwright, Dartford, Kent* †.

Table of the radii of wheels.

No. of Teeth	Radius in Inches.	No.	Radius.	No.	Radius.
10	3,236	42	13,382	74	23,562
11	3,549	43	13,700	75	23,880
12	3,864	44	14,018	76	24,198
13	4,179	45	14,336	77	24,517
14	4,494	46	14,654	78	24,835
15	4,810	47	14,972	79	25,153
16	5,126	48	15,290	80	25,471
17	5,442	49	15,608	81	25,790
18	5,759	50	15,926	82	26,108
19	6,076	51	16,244	83	26,426
20	6,392	52	16,562	84	26,744
21	6,710	53	16,880	85	27,063
22	7,027	54	17,198	86	27,381
23	7,344	55	17,517	87	27,699
24	7,661	56	17,835	88	28,017
25	7,979	57	18,153	89	28,336
26	8,296	58	18,471	90	28,654
27	8,614	59	18,789	91	28,972
28	8,931	60	19,107	92	29,290
29	9,249	61	19,425	93	29,608
30	9,567	62	19,744	94	29,927
31	9,885	63	20,062	95	30,245
32	10,202	64	20,380	96	30,563
33	10,520	65	20,698	97	30,881
34	10,838	66	21,016	98	31,200
35	11,156	67	21,335	99	31,518
36	11,474	68	21,653	100	31,836
37	11,792	69	21,971	101	32,155
38	12,110	70	22,289	102	32,473
39	12,428	71	22,607	103	32,791
40	12,746	72	22,926	104	33,109
41	13,064	73	23,244	105	33,427

* By the pitch is underſtood the diſtance between the centers of two contiguous teeth; and by the radius is underſtood the diſtance between the center of the wheel and the center of each tooth.

† Communicated by the author.

No

Table of the radii of wheels.

No.	Radius.	No.	Radius.	No.	Radius.
106	33,746	151	48,068	196	62,392
107	34,064	152	48,387	197	62,710
108	34,382	153	48,705	198	63,028
109	34,700	154	49,023	199	63,346
110	35,018	155	49,341	200	63,665
111	35,337	156	49,660	201	63,983
112	35,655	157	49,978	202	64,301
113	35,974	158	50,296	203	64,620
114	36,292	159	50,615	204	64,938
115	36,611	160	50,933	205	65,256
116	36,929	161	51,251	206	65,574
117	37,247	162	51,569	207	65,893
118	37,565	163	51,888	208	66,211
119	37,883	164	52,206	209	66,529
120	38,202	165	52,524	210	66,848
121	38,520	166	52,843	211	67,166
122	38,838	167	53,161	212	67,484
123	39,156	168	53,479	213	67,803
124	39,475	169	53,798	214	68,121
125	39,793	170	54,116	215	68,439
126	40,111	171	54,434	216	68,757
127	40,429	172	54,752	217	69,075
128	40,748	173	55,071	218	69,394
129	41,066	174	55,389	219	69,712
130	41,384	175	55,707	220	70,031
131	41,703	176	56,026	221	70,349
132	42,021	177	56,344	222	70,667
133	42,339	178	56,662	223	70,985
134	42,657	179	56,980	224	71,304
135	42,976	180	57,299	225	71,622
136	43,294	181	57,617	226	71,941
137	43,612	182	57,935	227	72,258
138	43,931	183	58,253	228	72,577
139	44,249	184	58,572	229	72,895
140	44,567	185	58,890	230	73,214
141	44,885	186	59,209	231	73,532
142	45,204	187	59,527	232	73,850
143	45,522	188	59,845	233	74,168
144	45,840	189	60,163	234	74,487
145	46,158	190	60,482	235	74,805
146	46,477	191	60,800	236	75,123
147	46,795	192	61,118	237	75,441
148	47,113	193	61,436	238	75,760
149	47,432	194	61,755	289	76,078
150	47,750	195	62,073	240	76,397

Table of the radii of wheels.

No.	Radius.	No.	Radius.	No.	Radius.
241	76,715	261	83,081	281	89,447
242	77,033	262	83,399	282	89,765
243	77,351	263	83,717	283	90,084
244	77,670	264	84,036	284	90,402
245	77,988	265	84,354	285	90,720
246	78,306	266	84,673	286	91,038
247	78,625	267	84,991	287	91,357
248	78,943	268	85,309	288	91,675
249	79,261	269	85,627	289	91,993
250	79,580	270	85,946	290	92,312
251	79,898	271	86,264	291	92,630
252	80,216	272	86,582	292	92,948
253	80,534	273	86,900	293	93,267
254	80,853	274	87,219	294	93,585
255	81,171	275	87,537	295	93,903
256	81,489	276	87,855	296	94,222
257	81,808	277	88,174	297	94,540
258	82,126	278	88,492	298	94,858
259	82,444	279	88,810	299	95,177
260	82,763	280	89,129	300	95,495

N. B. When the pitch is different from two inches, the radius of a wheel of any number of teeth, from 10 to 300 may be found from this table, by the Rule of Three; for as two inches (the pitch in the table) is to any radius in the table, so is any given pitch to the radius required.

For Example; let it be required to find the radius of a wheel of 100 teeth, when the pitch is $1\frac{1}{4}$ inches. The radius of a wheel of 100 teeth is, in the table, 31,836 inches. Accordingly we have 2 : 31,836 : : 1,25, to the number of inches in the radius required; which will be found 19,897 as by the Operation annexed.

2 : 31,836 : : 1,25
1,25

159180
63672
31836

2) 39,79500

19,8975

VII. *Account*

VII.

Account of the Pyrometer of Platina. By CITIZEN GUYTON.*

CITIZEN GUYTON preſented an inſtrument to the ſitting of the French National Inſtitute of the 26th Floreal laſt, intended to meaſure the higheſt degrees of heat of our furnaces.

Pyrometer for red heats, conſiſting of a lever of platina moved by the expanſion of a bar of the ſame metal; the whole being ſupported on a maſs of baked clay. *It is governed by the difference of the expanſions of pottery and platina.*

It conſiſts of a rod or plate of platina placed horizontally in a groove formed in a cake of hardened white clay. This plate is ſupported at one of its extremities on the part of the maſs which terminates the groove; the other end preſſes againſt a bended lever, whoſe longeſt arm forms an index to a graduated arc; ſo that the change of poſition of this index indicates the expanſion produced on the plate of metal by the heat.

Deſcription.

The cake of clay having been highly baked, leaves no cauſe to apprehend any contraction; and the expanſion which may take place during the ignition will only affect the very ſmall diſtance between the axis of motion of the index and the point of contact of the plate, that is to ſay, in ſuch a manner as rather to diminiſh the effect than to increaſe it.

All the parts of this inſtrument being of platina, neither fuſion nor oxidation are to be apprehended.

Dimenſions.

With reſpect to its dimenſions, the author conceives that in order to render the uſe of it commodious and accurate, they ſhould be reduced to ſuch as may be neceſſary to obtain ſenſible variations; it will then be rendered commodious by the facility with which it may be placed under a muffle or an inverted crucible, &c. and accurate, becauſe the probabilities of any accidental inequalities of the heat will be diminiſhed, which it is impoſſible to avoid to a certain extent, even in the midſt of a large maſs of fire.

Degree of accuracy.

The variations will be ſufficiently perceptible, if we can not only eſtimate, but correctly determine expanſions of the 200th part of a millimetre (about the 5000th part of an inch,) theſe the author obtains by the proportions of the inſtrument which he has himſelf adopted.

Dimenſions of the parts.

The rod or plate of expanſion is 45 millimetres (one inch and three quarters) in length, 5 in width (one fifth of an inch) and 2 in thickneſs (one thirteenth of an inch.)

* *Annales de Chimie*, No. 138. XLVI. 276.

The

The arm of the bended lever, which preſſes againſt the end of this rod, is 25 millimetres in length; (rather $2\frac{1}{2}$, or about one ninth of an inch) and the arm at right angles to it, or the index, which traverſes on the graduated arc, is 50 millimetres in length (one inch and eight tenths) or twenty times the length of the other. The ſpace traverſed by the diſplacing of the ſmall arm will be thus encreaſed in the proportion of 1 to 20.

As the long arm or index carries a nonius which divides each degree on the graduated arc into ten parts, we can diſtinctly obſerve the 200th of one of thoſe meaſures (referred to the bar itſelf.)

Laſtly, As the decimal diviſion of an arc of a circle of 50 millimetres radius, gives only 7.8538 deci-millimetres for one of its degrees, it is evident then that we may meaſure an expanſion of 0.078538 deci-millimetres, or of the 5730th part of the length of the radius.

In order to prevent the poſition of the index from being changed in removing the inſtrument from the furnace, a plate of platina is fixed ſo as to form a ſpring againſt its extremity.

The author has commenced a ſeries of experiments to determine the range of this pyrometer, to compare it with the pyrometric pieces of Wedgewood, and ſo to ſhew the degree of confidence it merits, the methods of uſing, and the caſes in which it may be uſefully employed in philoſophical reſearches and in the arts.

VIII.

Letter from Mr. Ezekiel Walker on the Proportion of Light afforded by Candles of different Dimenſions.

To Mr. NICHOLSON,

SIR,

Obſervations on a letter to the editor.

YOUR correſpondent, who has made ſome remarks on my experiments on candles, does not ſeem to have ſufficiently conſidered his ſubject; for had he paid attention to my paper on page 40 of the fourth volume of your Journal, he could not have advanced that "Though Mr. Walker aſſerts with conſiderable deciſion, that the light afforded by candles, is proportioned to the quantity of material conſumed, yet he has not given

given the detail of his experiments, but ſeems in ſome meaſure to have diſcovered this reſult by argument, from the ſuppoſed nature of the ſubject." *

Obſervations of light emitted by candles, and of the weights conſumed.

In the table in my paper above mentioned, the laſt column contains the diſtances of the candles from the wall, when the ſhadows were equal; and the fourth column contains the weights of thoſe candles conſumed in a given time, and theſe are all the data required for making the calculations, to ſhow whether my deduction is true or falſe. The mode of calculating ſeemed to me, at the time I wrote that paper, too eaſy to need any illuſtration, but as I now ſtand charged by your correſpondent, of having deduced a general law from doubtful principles, a further explanation becomes neceſſary.

Inveſtigation of the rules for computing.

To inveſtigate rules for this purpoſe, 1. Let M repreſent the mould candle, a its diſtance from the wall, on which the ſhadows were compared, x its quantity of matter conſumed in a given time, (t) and Q the quantity of light emitted by M in the ſame time: 2. Let m repreſent any other candle, b its diſtance from the ſame wall, and y its quantity of matter conſumed, in the time t.

Then as the intenſities of light are directly as the ſquares of the diſtances of the two candles from the wall, we have, as $a^2 : Q :: b^2 : \frac{b^2 \times Q}{a^2} =$ the quantity of light, emitted by m in the time.

Then let us ſuppoſe that the quantities of light are directly as the quantities of matter conſumed in the time t, and we have, As $x : Q :: y : \frac{y \times Q}{x} =$ the quantity of light emitted by m in that time, by hypotheſis.

Now, when $\frac{b^2 \times Q}{a^2}$ (Theo. 1.) is $= \frac{Y \times Q}{X}$ (Theo. 2.) the quantities of light of M and m are directly as their quantities of matter conſumed in any given time.

By theſe rules, the calculations contained in the following table, were made from the experiments mentioned at the beginning of this paper.

* See Philoſophical Journal, VOL. V. page 219.

No.

No of experiments.		Light by Rule I.	Light by Rule II.	2d Rule differs from the 1st.
1	No. 1. compd. with the mould	1.000	1.000	.000
2	No. 1. compd. with do.	1.000	1.000	.000
3	No. 1. compd. with do.	1.000	1.015	+ .015
3	No. 3. compd. with do.	1.196	1.125	− .071
4	No. 4. compd. with do.	1.196	1.226	+ .030
			The mean error of the 2d Rule	− .005

As the mean result given by the 2d rule, differs only 1 in 200 from the 1st, which is universally received as true, the 2d rule appears sufficiently exact for many practical purposes, where the properties of that light is concerned, which is produced by candles.

EZEKIEL WALKER.

Lynn Regis, 20th Sept. 1803.

Whether the experiments of a correspondent be accurate.

P. S. As to your correspondent's experiment, it does not appear so correct to me, as it appears to himself; for every one knows, that one end of a mould candle is thicker than the other, therefore if that gentleman made his experiment with the small end of his candle, he has estimated the quantity of light produced by a pound, too little; and if he made his experiment with the large end, his estimation is too great; and moreover, it may be doubted, whether the $\frac{1}{18}$ part of a pound of candles, can be so exactly ascertained by measuring as by weighing, even if the candles were perfect cylnders.

IX.

On the Compounds of Sulphur and Oxygen. By THOMAS THOMSON, *M. D. Lecturer on Chemistry in Edinburgh. From the Author.*

Three known compounds of sulphur and oxygen, 1. oxide, 2. acids.

IT is at present the opinion of Chemists that sulphur is capable of combining with three doses of oxygen, and of forming three distinct compounds, namely,

1. Oxide of Sulphur.
2. Sulphurous Acid.
3. Sulphuric Acid.

The

The firſt of theſe is ſuppoſed to contain a minimum, the third a maximum of oxygen. Of theſe three the conſtituents of the laſt only have been aſcertained with preciſion. It will be proper to begin with it, as the knowledge of its compoſition may be of ſervice in aſcertaining the conſtituents of the reſt.

I. *Of Sulphuric Acid.*

Sulphuric acid, compoſed of 61 ſulphur and 39 oxygen.

This acid has been lately analyſed with preciſion by Thenard and Chenevix. I have repeated their experiments with care, and have obtained for the mean reſult 39 per cent. of oxygen, which is only one half per cent. greater than the reſult obtained by Mr. Chenevix. This difference in the preſent ſtate of analyſis may be accounted altogether inſignificant. I ſhall conſider ſulphuric acid, then, as compoſed of

61	ſulphur
39	oxygen
100	

II. *Of Sulphurous Acid.*

Sulphurous acid.

Moſt of the properties of this Acid have been long known to chemiſts; but no experiments have been made to aſcertain the proportion of its component parts. Before I proceed to relate the reſult of mine, it may be worth while to deſcribe a few of the properties of Sulphurous Acid, which have not hitherto been ſtated with preciſion.

Water at 60° abſorbs nearly one-eleventh part of its weight of ſulphurous gas.

1. Fifty-three meaſures of ſulphurous acid gas were introduced into a graduated tube ſtanding over mercury, and one meaſure of water was thrown up. In five minutes 20 meaſures of gas were abſorbed, and in 24 hours the abſorption amounted to 33 meaſures. No farther abſorption took place in three days more; but on introducing the tube into water, the whole gas diſappeared, except a ſmall globule, which did not exceed 1-10th of a meaſure. During this experiment the thermometer at the time of obſervation deviated very little from 61°, and the barometer oſcillated from 29.55 to 29.77. Water then, at the temperature of 61°, abſorbs 33 times its bulk of this gas. Now, if with Lavoſier, we ſuppoſe a cubic inch of gas to weigh 0.63 grs. a cubic inch of water will abſorb 19.79 grains of ſulphurous acid, and 100 parts of water will abſorb 8.21 parts by weight.

2. A current

The impregnated water is intenſely acid. Sp. gravity 1.0513. Slight heat diſengages the acid.

2. A current of ſulphurous acid gas was paſſed through a large quantity of water till the liquid refuſed to abſorb any more. The taſte of the water thus ſaturated, was intenſely acid and ſulphureous, and its odour exceſſively ſtrong. The ſpecific gravity at the temperature 68° was 1.0513; the heat of the hand was ſufficient to occaſion an extrication of gas. When moderately heated, it frothed violently, and exhaled the denſe blue ſmoke which uſually indicates the preſence of ſulphurous acid. When boiled down in a retort to half its bulk, it loſt its ſmell, but ſtill continued ſlightly acid. Hence it obviouſly contained ſulphuric acid.

Analyſis of ſulphites. The ſolution of ſulphurous acid contains a ſmall portion of ſulphuric acid.

3. In analyſing the different ſulphites, I have not found barytes anſwer ſo well as I was led to expect from the experiments of Fourcroy and Vauquelin; the ſolubility of ſulphite of barytes in water is ſo conſiderable, that preciſion by means of it is ſcarcely to be looked for. But nitrate of lead yields with the alkaline and earthy ſulphites a white inſoluble powder of ſulphite of lead, which may be dried in the temperature of 300° without decompoſition, and is then compoſed of about

25 ſulphurous acid.
75 yellow oxide of lead.

100

One hundred parts of the above liquid ſulphurous acid yielded, with nitrate of lead, a precipitate indicating the preſence of 6.15 parts of ſulphurous acid. Another hundred parts, boiled down to one half in a retort, yielded, with muriate of barytes, a precipitate indicating the preſence of 0.34 ſulphuric acid. Therefore, 100 parts of my liquid ſulphurous acid contained about

5.81 ſulphurous acid,
0.34 ſulphuric acid,

6.15.

So that the ſulphuric acid amounts nearly to $\frac{1}{18}$ of the ſulphurous. The preſence of this acid is a proof of an affinity between ſulphurous acid gas and ſulphuric acid; for the gas was paſſed through an intermediate veſſel before it reached the water.

This contamination ſeems to render the water leſs capable of abſorbing ſulphurous gas.

4. The proportion of acid combined with water in the liquid ſulphurous acid was rather leſs than 7; yet, when water is plunged into a large column of gas, we have ſeen that it abſorbs

abſorbs rather more than eight parts of it by weight. Perhaps this difference was owing to the preſence of the ſulphuric acid in the liquid. For water, ſlightly acidulated with ſulphuric acid, abſorbs a ſmaller proportion of gas, than pure water.

Analyſis of ſulphurous acid.

5. After trying various experiments, in order to aſcertain the conſtituents of ſulphurous acid, I found the following method moſt to be depended on.

Sulphite of potaſh loſes ſome weight by a low heat: by ignition it loſes 22.3, and ſulphuric acid with the alkali are left.

Sulphite of potaſh was obtained by Berthollet's method. It is a fine white ſalt, the properties of which have been very fully detailed by Fourcroy and Vauquelin, though they have neglected to analyſe it.

When this ſalt is expoſed for a few minutes to a heat of 300°, it loſes 3.3 per cent. of its weight; and ſuffers no additional loſs, though the heat be continued for an hour. When heated to redneſs in a platinum crucible, it decrepitates, becomes of an opake white, a blue flame iſſues from below the lid, and, on taking off the cover at that inſtant, the ſalt may be obſerved of a glowing red heat in the middle. When this glow diſappears, the ſalt will be found to have ſuſtained a loſs of 22.3. per cent. and it loſes no more, though melted, and kept half an hour in fuſion. On evolving, it ſplits into the fine thin tranſparent plates, which diſtinguiſh ſulphate of potaſh in the ſame circumſtances. When this reſidue is diſſolved in water, and treated with muriate of barytes, this precipitate of ſulphate of barytes obtained, when dried and heated to redneſs, weighs 95.5, indicating the preſence of 22,92 ſulphuric acid. Suppoſing with M. Chenevix, that ſulphate of barytes contains 24 per cent. of ſulphuric acid: hence it follows that ſulphate of potaſh is compoſed of

22.30 volatile matter
22.25 ſulphuric acid
55.45 potaſh

100.00

The volatile matter is ſulphurous gas, with ſome ſulphur and a little water.

When 100 grains of ſulphate of potaſh were expoſed to the heat of a lamp in a retort with a very long beak, fitted to a mercurial air holder, they decrepitated and aſſumed the appearance of an opake white powder: 18 cubic inches of gas were extricated, and ſulphur, with a little water, was volatilized into the beak of the retort; the gas was abſorbed by water, and had the uſual ſmell of ſulphurous acid: the retort had

had loſt 15.2 grains of weight. The ſulphur being carefully collected, was found to weigh 5.1 grains. When burnt, it left 0.1 of reſiduum, which ſeemed to be ſulphurate of iron, for it gave a yellow colour to muriatic acid ;* the water volatilized could not be weighed, but I eſtimate it at 2 grains. The experiment ſhews us what the volatile matter is which is drawn off when ſulphate of potaſh is heated to redneſs. It is compoſed of

15.2 ſulphurous acid
5.1 ſulphur
2.0 water

22.3

The ſalt which remained in the retort being diſſolved, and treated with muriate of barytes, gave a precipitate which indicated the preſence of 23.2 of ſulphuric acid. Hence ſulphate of potaſh is compoſed of

Component parts of ſulphite of potaſh.

23.2 ſulphuric acid
15.2 ſulphurous acid
5.1 ſulphur
54.5 potaſh
2.0 water

100.0

But it is obvious that, before the application of heat, the firſt three conſtituents together conſtituted ſulphurous acid. Hence ſulphite of potaſh is compoſed of

43.5 ſulphurous acid
54.5 potaſh
2.0 water

100.0

Explanation of the action of heat on ſulphite of potaſh.

This analyſis enables us to trace the changes produced upon ſulphite of potaſh by heat. A temperature of 300° ſeparates the water and a ſmall portion of ſulphurous acid, which ſeems more looſely combined; for the ſalt, in conſequence, loſes its ſmell ; an increaſe of heat occaſions a ſeparation of a portion of the acid, unaltered ; the remainder divides itſelf into two parts, namely, ſulphuric acid, which remains combined with the potaſh, and ſulphur, which ſublimes. Hence

* I have never yet burned ſulphur, without obſerving traces of a ſimilar reſiduum.

we learn, that ſulphurous acid is compoſed of 23.2 ſulphuric acid, and 5.1 ſulphur, which gives us

82 ſulphuric acid
18 ſulphur
100

But 100 parts of ſulphuric acid contain 39 of oxygen; therefore 82 contain nearly 32. Hence ſulphurous acid is compoſed of

68 ſulphur
32 oxygen
100

Fourcroy affirms, that ſulphurous acid contains only about 15 per cent. of oxygen, which is leſs than one half of the reſult juſt given. But he quotes no experiment in proof of his aſſertion. In all probability it was a mere gueſs.

Sulphurous acid is moſt probably ſulphur combined with ſulphuric acid.

6. The phenomena which attend the acidification of ſulphur and the decompoſition of ſulphurous acid, render it probable that ſulphurous acid is rather a compound of ſulphuric acid and ſulphur, than of ſulphur and oxygen.

Sulphur and ſulphuric acid combine with great facility. For if we form them into a probe, a very moderate heat is ſufficient to convert the whole into ſulphurous acid gas.

Whenever ſulphur is acidified, a portion of ſulphuric acid always makes its appearance in whatever way the proceſs is conducted. Such at leaſt has been the conſtant reſult of my experiments.

When ſulphur is expoſed to the heat of an Argand lamp in a retort connected with a mercurial air holder, it melts and ſublimes at firſt rapidly, but much more ſlowly, when the proceſs has continued for ſome time. In a retort, whoſe capacity was 63 cubic inches, four hours elapſed before $\frac{1}{4}$ oz. of ſulphur was ſublimed into its neck. A conſiderable quantity of air was driven over; but on allowing the veſſels to cool, the whole returned again, except 3 cubic inches. So that, by the operation, the air in the retort had increaſed about $\frac{1}{20}$th part. It ſmelt very pungently of ſulphurous acid. When agitated in water, a ſmall portion of it diſappeared. The water did not acquire a perceptible taſte, but it precipitated muriate of barytes even after being boiled for ſome time. A portion of this air, after being well waſhed, was left in contact with a ſtick of phoſphorus over water. Its bulk was di-

miniſhed 17 per cent. Hence it had loſt 5 per cent. of oxygen by the action of the hot ſulphur on it. Here we ſee the ſource of the acidification of the ſulphur during its ſublimation. From this experiment we are authorized to conclude that both ſulphuric and ſulphurous acids may be formed merely by heating ſulphur in common air, without any ſenſible combuſtion.

Sulphur, whenever ſublimed, is in part acidified.

Sulphuric acid ſeems to be formed whenever ſulphur is ſublimed. For every ſpecimen of flowers of ſulphur which I have had an opportunity of examining, contained that acid. If common flowers of ſulphur be boiled in water, the liquid always precipitates muriate of barytes. But flowers of ſulphur, when once they have been well waſhed and dried, communicate no ſuch property to water. If we now ſublime theſe very flowers a ſecond time, water in which they are boiled, precipitates muriate of barytes, as at firſt.

Combuſtion of ſulphur under a jar over water.

When a red hot glaſs capſule is rapidly placed on a pedeſtal of ſtanding water, ſulphur thrown into it, and a glaſs jar ſuddenly put over it, the combuſtion of the ſulphur continues for a conſiderable time; denſe bluiſh-white fumes fill the jar, and at laſt conceal the flame completely. The ſmoke ſoon ſubſides when the combuſtion is over, and the water riſes ſlowly in the jar. By this proceſs, the air in the jar loſes uniformly 8 per cent. of oxygen; it retains the ſmell of ſulphurous acid, even though allowed to remain over water for a week. But the ſmell diſappears in an inſtant, if the air be paſſed through water. A portion of the water over which the jar ſtood, being treated with muriate of barytes, yielded a precipitate which weighed 8. An equal portion of the ſame water evaporated to one-fourth, yielded a precipitate which weighed 7.

Action of acids on the ſulphites.

7. The action of the more powerful acids upon the ſulphites deſerves attention, becauſe it ſerves to illuſtrate the nature of ſulphurous acid. This action has been deſcribed with conſiderable minuteneſs by Fourcroy and Vauquelin; but as the reſult of my experiments differs a little from theirs, a few obſervations may not be unacceptable to the chemical reader. To prevent tedioufneſs, I ſhall confine my remarks to ſulphite of potaſh.

Action of ſulphuric acid upon ſulphite of potaſh.

When ſulphite of potaſh is thrown into concentrated ſulphuric acid, a conſiderable heat is evolved, a violent effervescence takes place, and the ſalt loſes 48 per cent. of its

weight. The heat of boiling water renews the effervescence, and occasions a loss of weight, amounting to 2 per cent. more. So that sulphite of potash, when treated with sulphuric acid, loses uniformly the half of its weight. Yet it contains only 43.5 per cent. of sulphurous acid. The additional 6.5 parts may be ascribed perhaps to the escape of sulphuric acid along with the gas; for it can scarcely be doubted that there is an affinity between them. When the sulphuric acid solution is set aside, brilliant plates of super-sulphate of potash soon make their appearance in it.

Muriatic acid and sulphite of potash.

When sulphite of potash is thrown into muriatic acid, a violent effervescence ensues, but no increase of temperature; and the salt loses 34 per cent. of its weight. The heat of boiling water renews the effervescence, and occasions a farther loss of 16 per cent. making the whole loss amount to 50, as in sulphuric acid. From this experiment we see that muriatic acid does not expel the whole of sulphurous acid, unless assisted by heat: and in that case, a portion of the muriatic acid is driven off at the same time with the sulphurous. When the muriatic acid solution is set aside, beautiful arborescent crystals of muriate of potash make their appearance in it.

Oxy muriatic gas and sulphite of potash.

I dissolved 500 parts of sulphite of potash in water, and putting the solution in a Woulfe's bottle, caused a current of oxy muriatic acid gas to pass through it; the gas passed afterwards through a second bottle of water connected to the first by a bent glass tube. After the process the bottles were set aside, till the green colour, occasioned by the oxy-muriatic acid, disappeared, and the fetid animal odour which usually succeeds that colour, was become perceptible. From the first bottle I obtained, by means of muriate of barytes, a precipitate which weighed 777 parts, indicating the presence of 37.3 per cent. of sulphuric acid. But as the sulphurous acid originally present amounted to 43.5 per cent. had it been wholly converted into sulphuric acid, not less than 48.5 per cent. of sulphuric acid would have been obtained, there was a loss then of 11.2 per cent. of course, 10.5 parts of sulphurous acid must have been dissipated by the action of the oxy muriatic acid. Accordingly the liquid in the second phial gave an abundant precipitate with muriate of barytes: and this precipitate, contrary to what I expected, consisted chiefly of

ſulphite of barytes; for the greater part of it was ſoluble in ſulphurous acid.

Nitric acid and ſulphite of potaſh.

When ſulphite of potaſh is thrown into concentrated nitric acid, a violent efferveſcence takes place, and much heat is evolved, the loſs of weight is 44.5; the liquid treated with nitrate of barytes, gives a precipitate, which indicates the preſence of 39.6 of ſulphuric acid. Hence we ſee that the loſs of weight during the efferveſcence was owing chiefly to the eſcape of nitrous gas.

When the acid is diluted, the efferveſcence is violent, but no heat is evolved, and the ſmell of ſulphurous acid gas making its eſcape is very perceptible. The loſs of weight is only 12 per cent. the reſiduum, treated with nitrate of barytes, gave a precipitate indicating the preſence of 43.2 per cent. of ſulphuric acid. Here we ſee that moſt of the loſs of weight was owing to the eſcape of ſulphurous acid: yet the greater part was converted into ſulphuric acid.

Compoſition of ſulphite of potaſh.

8. During the courſe of theſe experiments I had occaſion to examine the compoſition of ſulphate of potaſh; and as my reſults differ a little from thoſe ſtated by others, it will be proper to notice ſome of them in this place.

When ſulphite of potaſh is heated to redneſs in a platinum crucible, the reſiduum poſſeſſes the properties of ſulphate of potaſh. It may be fuſed without any loſs of weight, and when diſſolved and cryſtallized again, we obtain the ſame ſalt as at firſt: 100 parts of this ſalt precipitated by muriate of barytes, yields a precipitate which, after being heated to redneſs, weighs, at a medium, 96 parts, indicating about 23 per cent. of ſulphuric acid. Hence this ſulphate is compoſed of

23 acid
67 potaſh

100

When ſulphuric acid is ſuperſaturated by means of carbonate of potaſh, we obtain by evaporation the common ſulphate of potaſh of chemiſts: the ſame ſalt ſeparates in cryſtals during the purification of the potaſh of commerce. When the ſalt is reddened in a platinum crucible, it loſes 1.4 per cent. of its weight, and no more, though it be kept in fuſion. Diſſolved in water, and treated with muriate of barytes, it yields a precipitate which weighs 128.5; the mean of three experiments differing

differing from each other not more than 3.5 per cent.* Hence it contains 30.84 ſulphuric acid. This ſulphate, then, is compoſed of

30.84	acid
67.76	potaſh
1.40	water
100.00	

When ſulphite of potaſh is left for ſome months expoſed to the air, and then heated to redneſs, it yields with muriate of barytes a precipitate indicating about 38 per cent. of ſulphuric acid.

The ſuperſulphate of potaſh loſes 26 per cent. in a red heat, and the remaining 74 parts diſſolved in water, and treated with muriate of barytes, yield a precipitate indicating the preſence of 30.4 ſulphuric acid, whereas 100 parts of the ſuperſulphate diſſolved in water, without being previouſly heated, yield, with muriate of barytes, a precipitate indicating the preſence of 38.4 ſulphuric acid. Hence it follows that the ſalt is compoſed of

38.4	acid
43.6	alkali
18.0	water
100.0;	

or, abſtracting the water, of

46.4	acid
53.6	alkali
100.0	

III. *Oxide of Sulphur.*

External characters of ſulphur.

Sulphur uſually occurs in one or other of three ſtates; namely, 1. A whitiſh powder, formerly diſtinguiſhed by the name of *lac ſulphuris*. 2. In rolls or flowers of a greeniſh yellow colour. This is the ſulphur of commerce. 3. In the ſtate of a reddiſh yellow, pitchy ſubſtance. This is commonly employed for forming the caſts of medals, &c. known by the name of ſulphurs.

* Mr. Vauquelin affirms, in his Diſſertation on the Potaſh of Commerce, that this ſalt yields with muriate of barytes a precipitate amounting to 22°, the weight of the ſalt. My experiment differs very much from this ſtatement.

1. It

Lac ſulphuris, or white ſulphur, is ſulphur and water; yellow ſulphur is the pure.

1. It is well known that ſulphur, when firſt obtained by precipitation from any liquid, is always of a white colour, which gradually changes to greeniſh yellow when the ſulphur is expoſed to the open air. If this white powder, or *lac ſulphuris*, as it is called, be expoſed to a low heat in a retort, it ſoon acquires the colour of common ſulphur; and, at the ſame time, a quantity of water is depoſited in the beak of the retort. On the other hand, when a little water is dropt into melted ſulphur, the portion in contact with the water immediately aſſumes the white colour of *lac ſulphuris*. If common ſulphur be ſublimed into a veſſel filled with the vapour of water, we obtain *lac ſulphuris* of the uſual whiteneſs, inſtead of the uſual flowers of ſulphur. Theſe facts prove that *lac ſulphuris* is a compound of ſulphur and water. Hence we may conclude that greeniſh yellow is the natural colour of ſulphur. Whiteneſs indicates the preſence of water.

Sulphur rendered viſcid and dark-coloured by fuſion.

2. It has been long known, that when a conſiderable quantity of ſulphur is kept melted for ſome time in an open veſſel, it becomes viſcid, changes its colour to a dark violet, and acquires a kind of pitchy appearance. The nature of this change has not hitherto been examined by chemiſts. Fourcroy, indeed, affirms, that the ſulphur, in this caſe, is in the ſtate of an oxide. But the aſſertion does not ſeem to have been the reſult of any poſitive experiment.

Does not ſucceed in a ſhallow veſſel.

I have never been able to produce this change in the appearance of ſulphur by heating it in a flat diſh, where nothing impedes the volatilization, though I have kept it melted in a glaſs capſule on ſand, heated to 250°, for ten hours together. But the change takes place in a ſhort time, when a conſiderable quantity of ſulphur is kept melted in a crucible; and the greater the quantity employed, the ſooner the change is produced, and the more complete it is.

This ſuppoſed oxide is of a violet colour; is ſoft if poured into water; one-ſixth denſer than ſulphur; and tough.

When ſulphur, thus converted into a ſuppoſed oxide, is newly prepared, its colour is a dark violet, with the metallic luſtre; not very unlike newly-melted muriate of ſilver, when ſeen by reflected light. If it be thrown ſuddenly, while in fuſion, into water, it continues ſoft for a conſiderable time; and, as it hardens, the colour changes from purple to reddiſh yellow. When broken, it exhibits a fibrous fracture, compoſed of ſmall priſmatic cryſtals. Its ſpecific gravity was 2.325. It was very tough, reſiſting, with a good deal of ob-

ſtinacy, the action of the peſtle. The powder had a ſtraw yellow colour. Its properties differ, we ſee, from roll ſulphur, which is remarkably brittle, and whoſe ſpecific gravity does not exceed 2.

To aſcertain whether this ſuppoſed oxide really contained oxygen, I treated 100 parts of it with nitric acid till the whole was converted into ſulphuric acid. The proceſs was as tedious as the acidification of common ſulphur, by means of the ſame acid. By nitrate of barytes I obſerved a precipitate, which, after being reddened in a platinum crucible, weighed 667, indicating 160 parts of ſulphuric acid; the ſuppoſed oxide had abſorbed, of courſe, 60 parts of oxygen. Hence we have ſulphuric acid compoſed of

It ſeems to contain a ſmall portion of oxygen, though perhaps not enough to entitle it to the name of oxide.

62.5 ſuppoſed oxide
37.5 oxygen

100.0

But 100 part of pure ſulphur would have abſorbed nearly 64 of oxygen, and formed 164 of ſulphuric acid. Hence it follows that the ſuppoſed oxide is compoſed of

97.6 ſulphur
2.4 oxygen

100.0

Though the reſult of a ſimilar experiment was nearly the ſame; yet the proportion of oxygen is certainly too ſmall to authorize us, in the preſent ſtate of chemical analyſis, to conclude that the ſuppoſed oxide really contains $2\frac{1}{2}$ per cent. of oxygen: for ſo ſmall a deviation from the compoſition of ſulphuric acid, by acidifying common ſulphur, as $2\frac{1}{2}$ per cent. may, very probably, be owing to an error of analyſis. At the ſame time the uniformity of my reſults inclines me to believe that this ſuppoſed oxide of the French chemiſts really contains ſome oxygen.

3. As no ſatisfactory reſult was likely to be obtained by expoſing ſulphur to heat and air, it became neceſſary to try the effects of thoſe chemical agents which are capable of communicating oxygen to other bodies. Sulphuric acid could not be uſed, becauſe ſulphur converts it into ſulphurous acid; the effect of nitric acid was well known; but the action of oxymuriatic acid had not been tried. Some of the foreign chemiſts, indeed,

Oxigenation of ſulphur by compound agents.

indeed, affirm that ſulphur takes fire when plunged into that gas; but they muſt have, ſome how or other, deceived themſelves.

Oxigenation by oxymuriaticacid. Sulphur expoſed to ox. mur. acid gas was converted into the *new product of a fine red liquid.*

I connected three Woulfe's bottles in the ſame manner, by means of glaſs tubes; furniſhed each with Welter's tubes of ſafety. The firſt contained an ounce troy of pure dry flowers of ſulphur; the ſecond was filled two-thirds with diſtilled water; and the third with a weak ſolution of cryſtallized carbonate of potaſh. A current of oxymuriatic acid gas was made to paſs through theſe bottles in the uſual way. The proceſs laſted a conſiderable time. The firſt bottle was ſoon filled with the greeniſh fumes of the gas; the ſulphur gradually became moiſt and doughy, and the particles of it which adhered to the ſides began to trickle down in drops; its colour changed to orange, and at laſt, a fine red liquid made its appearance. The whole of the ſulphur was gradually converted into this liquid. I then ſtopt the proceſs. Abundance of gas had paſſed through all the bottles: the water in the ſecond was, at one time, quite milky, but it recovered its tranſparency before the proceſs was finiſhed. Dots of ſulphur were depoſited along the glaſs tubes which connected the firſt and ſecond phials; but none in that which connected the ſecond and third, the ſolution in the third phial effervеſced preciſely as in the uſual proceſs for preparing hyper-oxymuriate of potaſh. The gas which eſcaped was carbonic acid. No oxymuriatic acid gas could be diſtinguiſhed by its ſmell in the ſmall capſule of water into which a tube iſſuing from the third bottle was plunged.

— denominated *ſulphureted muriatic acid.*

As the red liquid obtained by this proceſs has never before been examined by chemiſts, and, as it differs very much in its properties from all other ſubſtances at preſent known, it will be neceſſary to diſtinguiſh it by a peculiar name, I ſhall call it *ſulphureted muriatic acid*, till ſome better name be thought of.

— more than double the weight of the ſulphur.

This liquid amounted to $1\frac{5}{8}$ ounce meaſures, excluſive of what adhered to the ſides of the bottle; its ſpecific gravity was 1.623. It amounted, therefore, to 2.63 ounces, or more than twice the weight of the ſulphur, excluſive of what had been volatilized during the proceſs.

It is green by tranſmitted light.

Sulphureted muriatic acid is perfectly liquid; its colour is a fine red, intermediate between ſcarlet and crimſon. When ſtreaks of it run down the inſide of the phial, they appear green by tranſmitted light.

When

When expoſed to air, it ſmokes at firſt almoſt as intenſely as the ſmoking oxymuriate of tin of Lebavius; but the intenſity gradually diminiſhes, and, at laſt, reſembles that of the moſt concentrated muriatic acid a little heated. It is very volatile, diſappearing very rapidly when expoſed to a moderate heat.

It ſmokes, and is very volatile; taſte acid; reddens blue colour; gives white fumes by approach of ammonia; and when dropped into water, leaves a film of ſulphur, and gives yellow acid, ductile, inſoluble flakes that continue ſoft in the air.

Its ſmell has a ſtrong reſemblance to that of ſea-plants, but is much ſtronger. The eyes, when expoſed to its fumes, are ſoon filled with tears, and acquire the ſame painful feeling as when expoſed to the ſmoke of wood or peat.

Its taſte is ſtrongly acid, hot, and bitter, affecting the throat with a painful tickling.

It converts vegetable blue papers to red; but the change takes place ſlowly, unleſs the paper be dipt into water; the paper is not corroded unleſs heat be applied.

When brought near a phial of ammonia, denſe white fumes of muriate of ammonia make their appearance. If it be held above a ſolution of nitrate of ſilver, yellow flakes precipitate in abundance.

If a drop of ſulphureted muriatic acid be let fall into a glaſs of water, the ſurface of the water becomes immediately covered with a film of ſulphur; a greeniſh red globule falls to the bottom, which remains for ſome time like a drop of oil, but at laſt is converted into yellow flakes; theſe flakes have an acid taſte, which they do not loſe, though allowed to remain in water for ſeveral days; they are very ductile, and continue ſo, though left expoſed to the air.

4. To aſcertain the conſtituents of this liquid I agitated 110 parts of it in a very weak ſolution of potaſh, and then threw the whole on a filter: a yellow ductile ſubſtance was ſeparated, which adhered very ſtrongly to the filter; what I could ſeparate was put on a plate of glaſs, and dried gently in the open air. It weighed 40. This ſubſtance had a yellow colour, and reſembled half dry oil paint; its taſte was hot; it adhered ſo obſtinately to the finger, that ſeveral days elapſed before every trace of it diſappeared. When digeſted for ſome time in hot water, it fell into flakes of ſulphur, and the water acquired an acid taſte. The flakes weighed only 34, and poſſeſſed all the properties of common ſulphur. The water in which it had been digeſted yielded, with nitrate of barytes, a precipitate which weighed 8, indicating the preſence of 1.92 ſulphuric acid with nitrate of ſilver; the precipitate amounted to 16, indicating

Chemical examination of the red liquid or *ſulphureted muriatic acid*.

Chemical examination of the red liquid or *ſulphureted muriatic acid.*

ing 2.88 muriatic acid: but the precipitate had a browniſh tinge, of which muriate of ſilver is deſtitute. The 40 parts of yellow reſiduum then contained

34.00	ſulphur
1.92	ſulphuric acid
2.88	muriatic acid
1.20	water or loſs
40.00	

Seventy parts ſtill remain to be aſcertained. They muſt exiſt in the ſolution of potaſh. This ſolution, ſuperſaturated with nitric acid, and treated firſt with nitrate of barytes, and next with nitrate of ſilver, yielded precipitates indicating the preſence of 4.8 ſulphuric acid and 36.45 of muriatic acid. The reſiduum yield no farther precipitate with ſilver; but, when evaporated to dryneſs, ſome yellow cryſtals were obtained, which gave traces of ſulphur, but in too ſmall quantity to be weighed. This analyſis gives us the following proportions:

35.00	ſulphur
39.33	muriatic acid
6.72	ſulphuric acid
81.05	
28.95	loſs
110.00	
or per cent. 31.82	ſulphur
35.75	muriatic acid
6.10	ſulphuric acid
73.67	
26.33	loſs

This enormous loſs was owing, at leaſt, in part, to the improbability of ſeparating from the filter, the whole ſulphureous maſs ſo as to weigh it. This induced me to have recourſe to the following method of aſcertaining the proportions of ſulphur in ſulphureted muriatic acid.

Remarks on the ſulphuret of muriatic acid.

When ſulphureted muriatic acid is thrown into warm nitric acid a very violent effervеſcence takes place, and the whole mixture is thrown, with a kind of exploſion, out of the veſſel. If the acid be cold, the effervеſcence is at firſt ſlow, but heat is very ſoon evolved, and the ſame effects produced. When the proportion of nitric acid is great, and the ſulphuret dropt

in

in very ſlowly, the efferveſcence continues moderate; nitrous gas and oxymuriatic gas being evolved, as was evident, from the ſmell. I diſſolved 100 parts of the ſulphuret in nitric acid. The liquid yielded, with nitrate of barytes, a ſolution which, when properly dried, weighed 282 parts, indicating 67.6 parts of ſulphuric acid; but 67.6 of ſulphuric acid contain 26.3 oxygen. Hence 100 parts of ſulphureted muriatic acid contain 41.3 of ſulphur.

Chemical examination of the red liquid or *ſulphureted muriatic acid.*

If a cryſtal of muriate of barytes be ſuſpended in ſulphureted muriatic acid, no precipitate takes place; neither was any obtained by agitating this ſalt in powder with the ſulphuret. Hence I conclude, that the ſulphuret does not contain ſulphuric acid; but that ſulphuric acid is formed whenever the liquid comes in contact with water. The oxygen cannot be ſuppoſed to have been previouſly united to the muriatic acid ſolution. For I find, by trial, that if the oxymuriatic acid gas be ſtill made to paſs through the ſulphuret, after it is once formed, ſulphuric acid immediately makes its appearance in it. The oxygen then muſt have been combined with the ſulphur; the whole of which was in the ſtate of an oxide. Whenever the ſulphuret is diluted with water, that oxide undergoes decompoſition, one portion of it abſtracting the whole of the oxygen from the other, ſo that ſulphuric acid and ſulphur make their appearance together.

If we ſuppoſe the proportion of ſulphur and ſulphuric acid obtained by the firſt analyſis to be that which is formed when the ſulphur is mixed with water, we ſhall have the oxide of ſulphur compoſed of

	31.82	ſulphur
	6.10	ſulphuric acid
or of	35.54	ſulphur
	2.38	oxygen
or per cent. of	93. 8	ſulphur
	6. 2	oxygen
	100. 0	

But the ſulphuret yielded to nitric acid 41.3 per cent. of ſulphur. Hence it muſt contain 44 per cent. of oxide of ſulphur. Sulphureted muriatic acid then contains

44.00	oxide of ſulphur
35.75	muriatic acid
20.25	loſs
100.00	

Chemical examination of the red liquid or *ſulphureted muriatic acid.*

The loſs is ſtill very conſiderable. Probably the greater part of it is owing to the preſence of water; the amount of which it is impoſſible to aſcertain.

The above analyſis conveys but an inadequate idea of the conſtitution of ſulphureted muriatic acid, becauſe the proportions of its conſtituents vary conſiderably, according to the proceſs. The longer the proceſs is continued the greater is the encreaſe of muriatic acid, and the ſmaller the proportion of oxide of ſulphur. I found a portion of ſulphuret thus formed to contain

47.1	muriatic acid
35.2	oxide of ſulphur
4.0	ſulphuric acid
86.3	
13.7	loſs

It was this laſt ſulphuret that was obtained in the proceſs in which the quantity of ſulphuret which I got, as ſtated above, was meaſured, the ſecond Woulfe's bottle contained a ſolution of muriatic acid and ſulphuric acid in water. Hence we ſee that the ſulphuret, after being formed, had been partly covered over by the oxymuriatic acid gas. The ſulphuric acid obtained, by means of barytes, amounted to 36 grains; the muriatic acid to 139 grains. The third vial contained no ſulphuric acid, but conſiſted of a mixture of muriate of potaſh, hyperoxymuriate of potaſh and carbonate of potaſh.

Remarks on the ſulphureted muriatic acid.

5. The ſulphuret of muriatic acid claims the peculiar attention of chemiſts, not only on account of its compoſition, which our previous knowledge would have induced us to conſider as impoſſible, but on account of the many remarkable properties whch it diſplays. As I mean to reſerve a full account of its properties for a ſubſequent paper, I ſhall ſatisfy myſelf at preſent with the following remarks.

Sulphuret of muriatic acid diſſolves phoſphorus.

1. Sulphuret of muriatic acid diſſolves phoſphorus cold with great facility. No efferveſcence takes place; the ſolution has a fine amber colour, and is permanent. When evaporated, the phoſphorus remains behind with a little ſulphur, and at laſt takes fire. When the ſolution is mixed with liquid potaſh, the whole becomes beautifully luminous, and phoſphuret of ſulphur is precipitated.

Sulphuret efferveſces with alcohol, and forms ether.

2. When mixed with alcohol, a violent efferveſcence is produced, ether is immediately diſengaged, and, what I did not

not expect: this ether is mixed with ſulphurous acid, and muſt be rectified in the ſame way as ſulphuric ether, which it reſembles in ſmell.

3. All the acids decompoſe this ſulphuret, ſulphur uſually precipitating, except liquid ſulphurous acid, which produces no change, and nitrous acid, which diſſolves and decompoſes it at the ſame time. is diſcompoſed by all acids;

4. The fixed alkalies dry produce with it a violent effervescence and a very high degree of heat. When ammoniacal gas is paſſed through it, the veſſel is filled with a fine purple ſal-ammoniacal ſmoke, the whole becomes ſolid and of a deep red colour; but when mixed with water, ſulphur is immediately precipitated. habitudes with alkalis;

5. This liquid precipitates ſilver of a yellow colour mixed with white, the white is a muriate of ſilver, the yellow is a compound of the oxides of ſilver and ſulphur. It becomes brown when dry. Nitric acid decompoſes it, diſſolving the ſilver and acidifying the ſulphur. precipitates ſilver yellow and white.

IX.

Further Experiments and Obſervations on the Efflorescences of Walls. In a Letter from Dr. BOSTOCK.

To Mr. NICHOLSON.

SIR,

SINCE I ſent you the analyſis of the ſaline efflorescence from the walls of Mr. Earle's houſe, inſerted in your journal for November laſt, I have had an opportunity of examining four other efflorescences obtained from brick walls, the particulars of which I ſhall now detail. Four efflorescences from different walls.

The firſt of theſe was preſented to me by my friend Dr. Rutter, who diſcovered it in conſiderable quantity on the top of the walls of his houſe juſt below the roof. This ſalt had in every reſpect the ſame external characters with the one which I had before examined, and upon ſubmitting it to the action of the ſame chemical re-agents, ſimilar reſults were obtained. In addition to the former experiments, I compared the effects produced upon it by pure ammoniac, and by the carbonate of ammoniac; the former threw down a copious The firſt proved to be pure ſulphate of magneſia.

preci-

precipitate, while the latter had no perceptible operation; a deciſive teſt of the exiſtence of magneſia, which was ſuggeſted to me by Mr. William Henry of Mancheſter. This ſalt appeared, therefore, to be a very pure ſulphate of magneſia.

The ſecond efflorescence was ſulphate of ſoda with indication of muriatic acid.

The ſecond efflorescence which I examined was obtained from the outer wall of a ſtable, which had been erected for ſome years. It differed on its appearance from the two former; inſtead of ſhooting out from the wall in ſpiculæ of conſiderable length, it appeared like a powder ſcattered over the ſurface, occupying diſtinct, round patches, ſo as in ſome degree to reſemble the growth of a grey lichen. The bricks on which this efflorescence appeared were in general of a ſofter texture than thoſe of the reſt of the wall. Though it occupied a conſiderable extent, it was difficult to collect it in any quantity, but I obtained ſufficient to ſubject it to the following experiments. The ſalt was diſſolved in warm water, filtered and cryſtallized; the cryſtals were very ſoluble at the common temperature of the atmoſphere. Muriate of Barytes added to the ſolution produced a copious precipitate; carbonate of pot-aſh, pure pot-aſh, pure ammoniac and oxalic acid were reſpectively added to the ſolution, but produced no effect. Nitrate of ſilver cauſed a precipitate, but only in ſmall quantity. From theſe experiments it appeared that the ſalt in queſtion conſiſted of the ſulphuric acid, mixed with a ſmall proportion of the muriatic, and combined with one of the fixed alcalies. From the form of its cryſtals I conceived that the ſulphuric acid was in this caſe united to ſoda, but the quantity of ſalt which had been procured, was not ſufficient to enable me to determine accurately from this circumſtance. A more deciſive teſt between the ſulphate of ſoda and the ſulphate of pot-aſh, is the property which the latter alone poſſeſſes of forming alum with the acid ſulphate of alumine. I accordingly prepared a quantity of this ſubſtance; to one portion of it ſulphate of pot-aſh was added, and to the other ſome of the ſalt under examination. By gentle evaporation and ſubſequent cooling, the firſt produced very evident cryſtals of alum, the latter only formed a confuſed maſs. This I conſidered as a ſufficient proof that this ſaline efflorescence was the ſulphate of ſoda.

Third efflorescence ſulphate

The third efflorescence was obtained from the inner walls of a brick houſe which was then erecting; it had all the internal cha-

characters of the ſaline effloreſcence from the ſtable, and when ſubmitted to the ſame chemical re-agents, differed from it only in exhibiting ſlighter traces of the muriatic acid; this ſalt was therefore a ſulphate of ſoda nearly in a ſtate of purity.

of ſoda nearly pure.

The walls of the ſalt water baths in this town are covered with a ſtucco, which is in ſeveral places bliſtered and moulder-ing away. The parts of the plaiſter which are decaying, are covered with a copious effloreſcence, which has the appearance of a fine white down. Some of this I collected and examined. It was diſſolved in warm water and filtered; the ſolution was not capable of being regularly cryſtallized, but formed a white maſs, eaſily ſoluble, poſſeſſing the acrid taſte of a fixed alcali, and affecting the colour of teſt papers in the ſame manner. A briſk efferveſcence was excited by the addition of an acid, and from this circumſtance and the effect of the different re-agents, I conceived it to be one of the fixed alcalies. In order to determine whether it was an uncombined alcali, and to which of theſe bodies it ought to be referred, I aſcertained what quantity of the ſulphuric acid was requiſite to ſaturate a known weight of the ſalt, and afterwards, employing the ſame acid, compared it with the quantity which the ſame weight of alcali required. As the ſalt had not attracted any moiſture from the atmoſphere during a period of ſome weeks, I concluded it to be ſoda, and I accordingly found that the ſame quantity of ſulphuric acid ſaturated equal weights of ſoda, and of the ſalt under examination; the ſolution being ſlowly evaporated, formed well-marked cryſtals of the ſulphate of ſoda. That part of the walls of the bath on which the ſalt had effloreſced in the greateſt quantity, was out of the reach of the immediate action of the ſea water; but it is probable that the ſand of the ſhore had been mixed with the lime, for by examining a quantity of water which had been digeſted upon a portion of the plaiſter, it yielded a very copious precipitation by the nitrate of ſilver, and this rendered it highly probably that the mortar contained the muriate of ſoda. It might therefore be conjectured that the ſoda in this caſe was accidentally formed by the ſame proceſs which, according to Mr. Accum's account in the 2d vol. of the Journal, p. 243, is employed deſignedly in Pruſſia for obtaining it, by the decompoſition of common ſalt.

Fourth effloreſcence, on the ſtucco of a ſea-water bath, was carbonate of ſoda,

Probably formed by proceſs formerly deſcribed from ſea water.

I ſhall

Inveſtigation of the origin of the ſulphate of magneſia on the bricks of Mr. Earle's houſe.

I ſhall conclude this communication by detailing to you the progreſs which I have made in inveſtigating the origin of the ſulphate of magneſia, which appeared in Mr. Earle's houſe. Before proceeding farther, it will be proper to obſerve that the efflorescence was here altogether confined to the bricks, the mortar which united them being entirely free from it, and that rain water only had been employed in tempering the clay. It remained therefore to examine with accuracy, whether any ſalt, ſoluble in water, exiſted ready formed in the clay, and what were the component parts of the clay itſelf. To aſcertain the firſt of theſe points, 60 grains of the clay powdered and dried were well waſhed with boiling water; the water was filtered and evaporated, and the reſiduum carefully collected; it did not weigh $\frac{1}{4}$ of a grain. It was re-diſſolved in water; it produced a copious precipitate with the muriate of barytes, and the nitrate of ſilver; a very faint cloud with oxalic acid and with pure pot-aſh; ammoniac produced no effect. The ready formed ſalts appeared therefore to be the ſulphate and muriate of a fixed alcali, with a minute portion of the muriate of lime, the whole however exiſting in very ſmall quantity.

The clay contained a portion of muriate and ſulphate of alkali,

And a larger portion of magneſia.

The clay itſelf was next examined; it was found to conſiſt principally of ſilex and alumine in the proportion of about three to one; the quantity of lime was very ſmall, though its exiſtence was detected by the oxalic acid; its colour proved that it contained iron, and I alſo found that about five parts in the 100 conſiſted of magneſia. From this examination it appeared that one of the component parts of the ſalt exiſts in the clay; I attributed the formation of the ſulphuric acid to the ſulphur which is frequently met with in our coals, and which I conceived might unite with oxigen during the burning of the bricks. I attempted to put this action to the teſt of experiment, and accordingly I formed a paſte of pipe clay and calcined magneſia in the proportion of 95 to five; this was placed in a crucible, ſurrounded with ſmall coal mixed with a quantity of ſulphur; the crucible was then kept for ſome time in a ſtrong heat. I was not able to detect the preſence of the ſulphuric acid in the clay that was thus baked, but ſo many circumſtances might actually take place in the formation and burning of the bricks, which we have it not in our power to imitate in the laboratory, that I do not conſider the hypotheſis diſproved by my want of ſucceſs.

Experiment to ſhew whether ſulphur from the coals had given the acid? Doubtful.

In

In analyſing the clay I proceeded nearly upon the plan pointed out by M. Vauquelin in the 30th vol. of the Ann. de Chimie. Analyſis of the clay, &c.

This operation is however ſo tedious, and requires ſo much nicety in the management, that I made ſome attempts to aſcertain the exiſtence of magneſia in the clay by a ſhorter proceſs. The firſt method which I employed was ſuggeſted by an obſervation of Mr. Kirwan; he ſtates that alumine is ſufficiently diſcriminated from magneſia by the greater ſolubility of the latter in dilute ſulphuric acid; but it appears that the difference of ſolubility of theſe two ſubſtances cannot be employed as a teſt of the preſence of magneſia where it exiſts only in ſmall proportion; for I found that the ſulphuric acid diluted with above 200 times its weight of water, after being in contact with pure alumine for the ſpace of 10 minutes only, had acted upon the alumine ſo far that a precipitate was formed in the fluid by the addition of ammoniac.

The acetous acid is ſtated as poſſeſſing a much more powerful action over magneſia than over alumine; but upon trial the ſame objection occurred againſt its uſe as in the former inſtance.

The property which the magneſian ſalts poſſeſs of being decompoſed by pure ammoniac but not by the carbonate of ammoniac, ſeemed to offer a method by which the ſulphates of magneſia and of alumine might be ſeparated when mixed together in ſolution, and by which means conſequently the preſence of magneſia might be detected in the clay under examination. But I found that though the ſulphate of magneſia alone is not decompoſed by the carbonate of ammoniac, yet that when a mixed ſolution of alum and the ſulphate of magneſia is ſubjected to the action of the carbonate of ammoniac, both the alumine and the magneſia are precipitated, ſo that when the fluid is ſeparated by filtration, the addition of pure ammoniac produces no farther effect:

After I had made the unſucceſsful experiment related above reſpecting the formation of the ſulphuric acid, I received the laſt number of your Journal, containing a communication from Mr. Gregor, in which he gives an account of the production of the ſulphate of magneſia from the aſhes of pit-coal. He attributes the production of this ſalt to the decompoſition of the ſchiſtus and pyrites which are commonly found in coal, and

Mr. Gregor's experiment not applicable to the caſe in queſtion, as coal aſhes were not uſed.

and by heating a mixture of theſe ſubſtances he ſucceeded in forming the ſalt artificially. The ſucceſs of his experiment ſeems to prove the truth of the theory, at leaſt in the particular inſtance in which he obſerved the efflorescence: but in thoſe caſes where the ſalt evidently proceeds from the ſubſtance of the brick, and where the magneſia has been found to exiſt previouſly in the clay, the idea of Mr. Gregor appears leſs applicable, ſo far as regards the origin of the magneſia; but in both the proceſſes the ſulphuric acid is equally ſuppoſed to be derived from the pyrites. Mr. Gregor ſuggeſts that coal aſhes might have been mixed with the clay of which theſe bricks were formed; but I find upon enquiry that this was not the caſe.

I am,

Liverpool, Aug. 30, 1803. Your obedient ſervant, JOHN BOSTOCK.

X.

Philoſophical Obſervations on the Cauſes of the Imperfection of evaporating Furnaces, and on a New Method of conſtructing them, for the economical Combuſtion of every Deſcription of Fuel. By C. CURAUDAU, *correſponding Member of the Pharmaceutic Society of Paris* *.

On the combuſtion of fuel and conſtruction of furnaces.

NOTWITHSTANDING the attempts already made to introduce œconomy in the uſe of the combuſtibles neceſſary to the manufactures, we ſtill uſe them with conſiderable waſte. In all caſes a much greater quantity of fuel is conſumed than is needful to keep up the ebullition in evaporating furnaces, or to produce the requiſite temperature in furnaces for other purpoſes. It may be eaſily conceived that this ſuperfluous conſumption muſt in large eſtabliſhments be attended with great loſs, and muſt eventually tend to produce a ſcarcity of fuel in the market. It therefore becomes us for both reaſons to endeavour to prevent a ſcarcity, of which future generations might with juſtice accuſe us of being the authors, unleſs we ſeriouſly occupy ourſelves in ſearch of the methods of burning wood with more economy. Many very remarkable improve-

* From the *Annales de Chimie*, No. 138.

ments

ments have indeed been made in the conſtruction of furnaces within theſe few years, but they are only advances towards perfection, and are yet very far from being carried to the extent of which they are capable. This will no doubt be the caſe with the alterations I am about to propoſe; for theſe will enable us to make new obſervations, which moſt probably will lead to further alterations ſtill more important.

On the combuſtion of fuel and conſtruction of furnaces.

Concerning Evaporatory Furnaces.

The phyſical impoſſibility of raiſing the temperature in evaporating furnaces as they are at preſent conſtructed, is one of the cauſes which has always appeared to me moſt ſtrongly inimical to their improvement. For it muſt not be imagined that the intenſity of the heat will be in proportion to the quantity of matter in ignition, or that caloric will not be more copiouſly produced by the ſame quantity of combuſtible under certain circumſtances than others; as for example, when the temperature is already very high, the products of heat from a combuſtible ſo ſituated will be much more conſiderable than thoſe from the ſame combuſtible burned in a furnace, the temperature of which is conſtantly depreſſed by the evaporation of the liquid in the boiler.

To prove that it is only by virtue of a temperature already elevated that we can obtain an advantageous combuſtion, I ſhall take Argand's lamp, which will afford a comparative inſtance on a ſmall ſcale of the effect produced by the intenſity of heat during combuſtion. When theſe lamps have their glaſs chimney, they afford a very brilliant light, and the oil will emit no ſmoke. But if the chimney be taken off, the oil will immediately burn duller, the light will be leſs intenſe, and the wick will give out ſmoke. This effect ſhews that it is the current of air in the chimney, and the heat it keeps up round the wick, which contributes to the effect of the combuſtion. What ſtill adds weight to this opinion is, that the perfection to which this kind of lamp is brought depends principally on the form and proportions of its glaſs chimney.

This example muſt then naturally lead us to think that evaporating furnaces, as they are made at preſent, cannot advantageouſly promote combuſtion, ſince the bottom of the boiler, which is continually kept at the ſame degree of heat by the evaporation of the liquid in boiling, conſtantly prevents the

On the combuſtion of fuel and conſtruction of furnaces.

riſe of the temperature, whence it reſults that the heat which is inſufficient to produce a complete combuſtion of the inflammable particles, will rather produce gaziſication than oxigination. This volatilization of the particles of combuſtible bodies which eſcape combuſtion, and which paſs ſucceſſively into the ſtate of permanent gaſes, will alſo abſorb a quantity of heat neceſſary to their gaſeous conſtitution, which, together with the effects of the current of air, will tend to lower the interior temperature of the furnace, and to impede the proceſs of combuſtion.

Theſe remarks, which perfectly agree with all the phenomenæ of combuſtion, ſhew that the oxigen of the atmoſphere does not act with much efficacy on combuſtible bodies, except when they are immerſed in it at an high temperature, and that to apply an intenſe and uniform heat to an evaporating furnace without loſs of the combuſtible, it ſhould be produced in a fireplace, having a current of air, and ſo far diſtant from the boiler that the temperature may be raiſed gradually and at pleaſure. By this means all the particles of the combuſtible matter will be in a ſtate favourable to their oxigination; and the whole quantity of radiant heat produced by the reaction of the oxigen upon the combuſtible, will be diſengaged and employed without loſs.

That which under ſimilar circumſtances conduces ſtill farther to encreaſe the action of the oxigen, is its continual renewal. For the higher the temperature of a furnace is raiſed the more eaſily the outer air will enter; and ſo likewiſe when the ignition is carried to a high degree, it becomes neceſſary and advantageous to check the current of air, not by cloſing the door or lower opening of the furnace, as is generally done, but rather by contracting or even cloſing the upper aperture of the chimney. By this means the heat becomes concentrated in the body of the furnace, and has no other paſſage than through the liquid in the boiler.

This remark on the method of checking the current of air by the top of the chimney, may alſo be applied to furnaces of fuſion, and in caſes where it is required to maintain the heat of a metal without expoſing it to the oxigenating action of a current of air in a ſtate of ignition.

General

General Remarks on the Conſtruction of Furnaces.

That part of the fire-place which is to ſupport the greateſt heat, ſhould be made of very refractory bricks. The beſt cement or mortar for bricks, in all caſes where a bad conductor of heat is required, is a mixture of equal parts by meaſure of tan and clay. The tan prevents the cement from cracking, and produces an adheſiveneſs which, when dry, gives it a great degree of firmneſs.

Furnaces may alſo be conſtructed with this mortar, and on the ſame principles with thoſe of evaporation, which I am about to deſcribe.

Furnaces intended for ſtrong heat, ſhould be externally covered with a thick wall, conſtructed with the mortar of tan. By this means very little heat will be loſt. All furnaces ſhould be ſo conſtructed as to have the power of cloſing the upper aperture of the chimney at pleaſure, in order to check the combuſtion, and concentrate the heat within the furnace, whenever this becomes neceſſary. When the temperature is very high, it is particularly neceſſary to regulate the iſſue of the current of air ſo as to prevent its too ſpeedy circulation through the furnace, which, in certain caſes, is prejudicial to the ſucceſs of the operation.

By uniting all theſe conditions in furnaees, a certain ſaving of one fourth of the fuel will be made, and the combuſtion will be produced without any appearance of ſmoke. I inſiſt more particularly on this remark, becauſe it is clearly and phyſically proved that no combuſtible can be completely burned if ſmoke be produced.

Deſcription of an Evaporating Furnace, in which the Temperature may be raiſed at Pleaſure.

Deſcription of a furnace for evaporation.

For common furnaces, the aperture of the vault A, Plate VI. ſhould be four decimetres (15$\frac{3}{4}$ inches) wide, by three decimetres and a half (13$\frac{3}{4}$ inches) in height; B is the part of the vault in which the combuſtion is performed. This vault muſt be at leaſt two metres (6$\frac{1}{2}$ feet) in length. C repreſents a boiler of one metre and a half (nearly five feet) in diameter, and of the ſame depth; it is ſet in a brick furnace.

The interval from the bottom of the boiler to the baſe of the furnace muſt be at moſt one decimetre (about four inches.) It muſt be obſerved in the conſtruction of furnaces, that the brickwork ſhould be gradually ſloped towards the boiler, and to reduce

duce the ſpace to about three centimetres (about an inch.) It muſt be thus continued to within one decimetre of the edge of the boiler; and muſt then be brought into contact with it. D is an aperture of two decimetres wide by one in height (about eight inches by four) communicating with E. But at the ſide of the angle *a*, this paſſage for the heat muſt be made of one metre (three feet three inches) in width, by one decimetre (about four inches) in height, and this proportion continued to the aperture E.

F is a ſecond boiler, intended to be heated by means of the exceſs of heat from the firſt; many others may be applied in ſucceſſion, if required. G is an aperture with the ſame proportions as D. At the angle *b*, it muſt be obſerved to make the aperture of the chimney five decimetres by two ($19\frac{1}{2}$ inch. by $7\frac{3}{4}$ inch.) and to continue this proportion to about two thirds of its height. The aperture may then be contracted ſo that, at its upper extremity, it may not be leſs than one decimetre by three (about four inches by twelve.) This part of the chimney ſhould be ſo conſtructed as to be able to cloſe it conveniently, when required.

XI.

Correction of a Miſtake in Dr. Kirwan's Eſſay on the State of Vapour in the Atmoſphere. *By* Mr. DALTON.

To Mr. NICHOLSON.

SIR,

Miſtake of Dr. Kirwan in quoting Mr. Croſthwaite as authority for a maximum height of the clouds.

I TAKE the liberty of requeſting you to correct a miſtake in Dr. Kirwan's eſſay on vapour, copied in your laſt number, page 246. In treating upon the height of clouds, he obſerves, "In lat. 54°, in Cumberland, Mr. Croſthwaite obſerved none "lower than 2700 feet, and none higher than 3150, in the "courſe of ſeveral years." For this he refers to my meteorological obſervations, page 41. The facts there ſtated, however, are ſo very different from thoſe above mentioned, that I conclude Dr. Kirwan has not ſeen the book, and has been miſinformed. The account referred to is, that Mr. Croſthwaite obſerved the heights of the clouds uſually three times a day for five years, by remarking their interſection with Skiddaw (a high mountain near Keſwick.) The reſult was,

Clouds from 0 to 100 yards above Derwent Lake,	10 times.
100 - 200	42
200 - 300	62
300 - 400	179
400 - 500	374
500 - 600	486
600 - 700	416
700 - 800	367
800 - 900	410
900 - 1000	518
1000 - 1050	419
above 1050	2098
Total	5381

His obſervations could not be particular above 1050 yards, that being the perpendicular height of Skiddaw. There is not therefore any *maximum* of height ſo much as hinted at; and the *minimum* is 0, or when the clouds reſt on the ground, an event occurring in every part of Great Britain two or three times a year. Were we to form a conjecture from the above obſervations relative to the greateſt height at which clouds are formed in this country in ordinary, it would be about 1 mile; but in ſummer they are probably ſometimes $1\frac{1}{2}$ mile above the level of the ſea.

No maximum was given or aimed at by that author's obſervations.

Conſidering the great ſervice that Dr. Kirwan has rendered to meteorology and chemiſtry, and my own obligations to him on thoſe accounts, it is unpleaſant for me to ſignify diſſent from the doctrine he inculcates reſpecting the ſtate of vapour in the atmoſphere. At the ſame time that his intereſting ſeries of eſſays in the eighth volume of the Tranſactions of the Royal Iriſh Academy were in the preſs, my eſſays on the force of vapour from water and other liquids, both in a vacuum and in air, and on evaporation, publiſhed in the Manchеſter Tranſactions, Vol. V. Part 2, were alſo in the preſs. He holds the notion of a chemical ſolution of water in air; and I maintain that vapour ſubſiſts in air as it does in *vacuo*, conſtituting a peculiar atmoſphere, mixing but not combining with any of the gaſes of the compound atmoſphere. On my principle the denſity of the aqueous atmoſphere at any height is totally independent of the denſity of the compound maſs of air, and is to be aſcertained by knowing the denſity of vapour at the earth's

Obſervations, or ſhort ſtatement of Dr. Kirwan's and Mr. Dalton's theories of the ſtate of atmoſpheric vapour.

earth's ſurface, and its ſpecific gravity; in the ſame way as we would aſcertain the denſity of the oxygenous or azotic atmoſpheres, or one of hidrogen, at any given height, having the like *data*.

It has been a matter of ſurpriſe to me, that moſt or all of my eſſays publiſhed in the volume above mentioned, have been copied and circulated in one or other of our periodical publications, except thoſe two juſt mentioned, which appear to me by far the moſt important, and which ſeem too to have been conſidered as ſuch by the foreign journaliſts.

I am your's, &c.

J. DALTON.

Mancheſter, Aug. 22, 1803.

XII.

Cheap and effectual Method of ſecuring Beams of Timber in Houſes or elſewhere, which have been injured by the Dry Rot, or are decayed by Time. By Mr. JAMES WOART.*

Eaſy method of ſecuring decayed timbers in buildings, &c.

WHERE the ends of the girder are decayed by time, or injured by the dry rot, they are often taken out, and new ones put in their place, at a great expenſe: and if the dry rot is in the walls, the ends of the new girder will be in danger of it again: ſuch was the caſe at Eltham, in Kent, where in one houſe there were three new girders to one floor in the ſpace of twenty years; whereas my method will be found infallible, executed at much leſs expence, and not ſubject to the dry rot, becauſe the end of the girder may be cut off clear from the wall; and if an air grate is put on the outſide, ſo as to admit air to the end of the girder, it will remain ſafe from injury.

Plate IX. *Fig.* 2.†—A, ſhews the end of the decayed girder, with the braces applied upon it.

B B,

* Memoirs of the Society of Arts, 1802. A reward of twenty guineas was awarded to the inventor, who, in an introductory letter ſtates, that by the iron braces, of leſs coſt than 20l. he ſecured the houſe of Hannege Legg, Eſq. at Putney in Surry, which could not have been done by new beams without loſs, derangement, and charges to the amount of eight hundred pounds.

† There are two plates in the Tranſactions, the ſecond of which forms

Eaſy method of ſecuring decayed timbers in buildings, &c.

B B, the templets or wall-plates on which the girder reſts.

C C C C, one of the iron levers for raiſing and ſupporting the girder (there being a ſimilar one on the oppoſite ſide.) This lever is moveable on a pin D, which comes through a hole in the lever, diſtant about two feet from the end of the girder. This pin forms part of a collar E bedded in the girder. The lever is ſix feet long, three inches wide, and three fourths of an inch thick, and extends from the wall-plate along the ſide of the girder.

The extremity of the lever is moveable on another pin F, projecting through it from an upright iron G, bedded in the ſide of the girder, and carrying a nut and ſcrew, which act on a croſs plate H, through which the upright iron paſſes.

At the other end of the lever, next the templet, is an iron collar I, bedded in the girder, which collar may be raiſed or lowered at pleaſure, by means of the nut and ſcrew K, forming part of it; and by aid of the cap-plate L, which preſſes upon the lever, and alſo claſps it to the girder by its bend at L.

As *Plate* IX. *Fig.* 2, ſhows only one ſide of the girder, and, as has been before obſerved, there being alſo a ſimilar lever on the oppoſite ſide of the girder, their ſeparate parts, method of connecting them, and their mode of action, are more fully explained in *Plate* VII. *Fig.* 1, 2, 3, where the ſame letters are made uſe of to point out the ſeveral parts.

Fig. 1.—E, ſhows the whole of the collar to be bedded in the ſide and bottom of the girder, and the pins D D, on which the two levers are moveable.

Fig. 2.—The cap-plate H, the two upright irons G G, with their nuts and ſcrews, which act upon the extremities of the two levers by means of their pins F F.

Fig. 3.—The collar I, on which that end of the girder next the templet reſts, the ſides of which collar are bedded in the girder. C C are the claws or bended legs of the two levers which go into the templet. L is the cap-plate, K K are the nuts and ſcrews.

At Mr. Legg's houſe, where the levers above mentioned were applied, the beams of the roof were ſo decayed that the roof was in imminent danger, the bearings were entirely rot-

forms Plate VII. of our preſent number. The other unfortunately was neglected to be ſent to the engraver's, and the miſtake not diſcovered till too late. It will be given in our next.

ten,

Eaſy method of ſecuring decayed timbers in buildings, &c.

ten, and the beams were ſunk three fourths of an inch, and preſſing againſt the wall for ſupport; if there had not been a large cornice underneath, ſupported by brackets, the whole roof muſt have fallen.

To put them in order, I firſt put ſhores or ſupports under each end of the two beams, on which the double roof lay, and then forced the four ſhores at once, for the ſecurity of the roof, the work, and men. The iron levers, C, were then prepared, let into the templet, and fixed on each ſide of the beam, on the pins D, projecting from the collar E, bedded in the beam, about two feet from its end. When the whole apparatus was ready, on ſcrewing the nuts on the upright irons G, at the extremity of the levers, the beam was raiſed to its proper height with great eaſe, although it was ſuppoſed there was above two tons weight on each beam, on account of the lead gutter, and gutter-beam betwixt the double roof, and the rich ornamented ceiling attached to the joiſt, which was not in the leaſt deſtroyed except where the iron collar E was fixed, which was put up from the under ſide by cutting the ceiling the width of the collar. Theſe beams were ſo decayed, and ſo hollow, that the common method of bolting plank on each ſide of the beam would not have been ſafe; and if it could have been executed, the new planks would have been ſubject to the dry rot, and the roof ſtill in danger, which is now prevented, as the iron is not affected by it. The beam-ends were cut clear from the walls, and the beams are ſuſpended by means of the iron levers, whoſe feet reſt on the templets of the walls. An air grate was made, on the outſide of the wall, to admit a current of freſh air to the ends of the timbers. The roof is now much ſafer than when originally made, as the timber is ſecured from decay; and, owing to the collar E, the bearings are now two feet ſhorter at each end of the beam; the bearing on each beam being now, in the whole, four feet ſhorter than in its original ſtate.

After the beams were brought to their proper height, and the levers and ſcrews adjuſted, ſcrew-bolts were put into the timber, through holes purpoſely left in the lever, betwixt D and F, and the whole work thus perfectly ſecured.

At the other end of the girder, M, *Plate* IX. is ſhown another method of ſupporting timbers, where the ends are decayed.

The

The particular irons uſed in this way are ſhown in Plate VII. Fig. 4. N is a collar for the girder; O, an iron frame which reſts on the templet; P P, two nuts which raiſe the collar N. R R ſhow the clawed ends of the two bars of iron, extending under the girder, bedded therein, and ſcrewed to it at their extremities, about five feet diſtant from the templet. Eaſy method of ſecuring decayed timbers in buildings, &c.

Fig. 5, is one of the iron bars laſt mentioned.

S is the claw or lap which projects over the collar N. T is the place where it is ſcrewed into the girder.

Fig. 6 and 7. Plate VII. explain a third method of ſecuring decayed timbers.

Fig. 6, gives a ſide view of a decayed girder: *a*, repreſents the templet; *b*, an iron lever, ſix feet long, nearly ſtrait, being only cambered one inch, three inches wide, and three quarters of an inch thick; this lever extends along the ſide of the girder *c*, and is ſecured firmly to it by the ſide irons *d d d d*, which are two inches wide, and full half an inch thick, pointed at the ends. The higher ends of theſe ſide irons are driven into the girder, and the lower points paſs through holes in the lever into the lower part of the girder, and are held cloſe to the girder by ſtaples *e e e e*: the ſide iron next the templet may be fixed ſlanting, in order that it may enter ſounder wood. A claw, *f*, which is part of the lever, reſts on the wall plate *a*, and is bedded in it; an iron plate, *g*, lying under the girder, and let into it, paſſes through the lever at *h*, connecting it with a ſimilar lever on the oppoſite ſide, and which aſſiſts in the ſame way to ſupport the girder: *i* is a flooring joiſt, to ſhow how deep the levers are inſerted therein.

Fig. 7, ſhows the under part of the ſame girder; *b b*, are the bottoms of the two levers above mentioned, fixed to the girder by the ſide irons and ſtaples before deſcribed; *k k*, the broad feet of the levers which lie flat upon the wall plate; *f f*, the two claws projecting from the feet, in order to bed in the wall plate; *i i i i* are joiſts, partly cut through, to admit the iron levers to lie cloſe to the girder: *g* ſhows the iron plate or collar on which the girder bears; it is turned up an inch and a half at each end, to keep the levers cloſe to the ſides of the girder. This collar ſhould be made out of inch-bar iron, with points projecting from it, in the ſame manner as the collar at D D, *Fig.* 1, to connect it with the levers, by paſſing through holes made through them for that purpoſe.

To

To fix the levers, put a ſhore two feet ſix inches from the wall, under the girder, to ſupport it; then cut off the decayed end, and take out the templet, or part of the wall plate, if decayed; and put in a ſtone templet for the irons to reſt upon, with mortices in the ſtone to admit the claws of the lever: then fit the collar underneath the girder, two feet from the wall, to anſwer the holes in the lever; make an inciſion in the joiſts three-fourths of an inch wide, and three inches deep, to admit the levers; fix the levers on each ſide with the collar, ſo as to force up the levers together; then with ſlight ſhores force up the ends of both levers together, and fix the ſide-irons firm. The girder will thus be perfectly ſafe.

The templet or wall plates, on which the levers reſt, are made of Portland ſtone, three feet long, nine inches wide, and five inches deep, with inciſions or mortices made therein for the claws of the levers.

Certificates, confirming Mr. Woart's improvements, were received from the commiſſioners of the navy, from Mr. Joſeph Harris, ſmith, at Putney, and Mr. George Smith, ſurveyor, at Putney.

XIII.

Account of the Method of extreme Branch Grafting. By the Inventor WILLIAM FAIRMAN, *Eſq.**

SIR,

Introductory obſervations on the uſeleſs trees in orchards.

FROM much converſation with Mr. Bucknall on the idea of improving ſtandard fruit-trees, we could not but remark that in apple orchards, even in ſuch as are moſt valuable, ſome were to be ſeen that were ſtinted and barren, which not only occaſioned a loſs in the production, but made a break in the rows, and ſpoiled the beauty and uniformity of the plantation.

To bring theſe trees into an equal ſtate of bearing, ſize, and appearance, in a ſhort time, is an object of the greateſt importance in the ſyſtem of orcharding, and alſo for the

* In a letter to Charles Taylor, Eſq. Secretary to the Society of Arts, and inſerted in their Memoirs for 1802. For which the ſilver medal was awarded.

recovery

recovery of old barren trees, which are fallen into decay, not ſo much from age, as from the ſorts of their fruits being of the worn-out and deemed nearly loſt varieties.

Having long entertained theſe thoughts, and been by no means inattentive to the accompliſhment of the deſign, I attempted to change their fruits by a new mode of engrafting, and am bold enough to aſſert that I have moſt fortunately ſucceeded in my experiments; working, if I am to be allowed to ſay it, from the errors of other practitioners, as alſo from thoſe of my own habits. New method of grafting.

My name having ſeveral times appeared in the Tranſactions of the Society for the Encouragement of Arts, &c. and having the honour of being a member of that Society, I thought no pains or expence would be too much for the completion of ſo deſirable an improvement. Under theſe impreſſions, and having many trees of this deſcription, I made an experiment on three of them in March 1798, each being nearly a hundred years old. They were not decayed in their bodies, and but little in their branches. Two of theſe were golden pippins, and the other was a golden rennet. Each likewiſe had been paſt a bearing ſtate for ſeveral years. I alſo followed up the practice on many more the ſucceeding ſpring, and that of the laſt year, to the number of forty at leaſt, in my different plantations *.

The attempt has gone ſo far beyond my moſt ſanguine expectation, that I beg of you, Sir, to introduce the ſyſtem to the Society, for their approbation; and I hope it will deſerve the honour of a place in their valuable Tranſactions.

I directed the proceſs to be conducted as follows: Cut out all the ſpray wood, and make the tree a perfect ſkeleton, leaving all the healthy limbs; then clean the branches, and cut the top of each branch off where it would meaſure in circumference from the ſize of a ſhilling to about that of a crown piece. Some of the branches muſt of courſe be taken off where it is a little larger, and ſome ſmaller, to preſerve the canopy or head of the tree; and it will be neceſſary to take out the branches which croſs others, and obſerve the arms are left to fork off, ſo that no conſiderable opening is to be perceived when you ſtand under the tree, but that they may repreſent an Inſtructions for preparing the trees.

* The average expence I calculated at 2s. 6d. each tree.

uniform head. I muſt here remark to the practitioner, when he is preparing the tree as I directed, that he ſhould leave the branches ſufficiently long to allow of two or three inches to be taken off by the ſaw, that all the ſplintered parts may be removed.

Poſition of the grafts.

The trees being thus prepared, put in one or two grafts at the extremity of each branch; and from this circumſtance I wiſh to have the method called *extreme branch grafting*.

Cement and general preparation and management.

A cement, hereafter deſcribed, muſt be uſed inſtead of clay, and the grafts tied with baſs or ſoft ſtrings. As there was a conſiderable quantity of moſs on the bodies and branches of the trees, I ordered my gardener to ſcrape it off, which is effectually done when they are in a wet ſtate by a ſtubbed birch broom. I then ordered him to brush them over with coarſe oil, which invigorated the growth of the tree, acted as a manure to the bark, and made it expand very evidently; the old cracks were ſoon, by this operation, rendered inviſible.

All wounds ſhould be perfectly cleaned out, and the medication applied as deſcribed in the Orchardiſt, p. 14. By the beginning of July the bandages were cut, and the ſhoots from the grafts ſhortened, to prevent them from blowing out. I muſt here, too, obſerve, that all the ſhoots or ſuckers from the tree muſt enjoy the full liberty of growth, till the ſucceeding ſpring, when the greater part muſt be taken out, and few but the grafts ſuffered to remain, except on a branch where the grafts have not taken: in that caſe, leave one or more of the ſuckers, which will take a graft the ſecond year, and make good the deficiency. This was the whole of the proceſs *.

Great advantages of leaving the tree of its full ſize.

By obſerving what is here ſtated, it will appear that the tree remains nearly as large when the operation is finiſhed, as it was before the buſineſs was undertaken; and this is a moſt eſſential circumſtance, as no part of the former vegetation is loſt, which is in health fit to continue for forming the new tree.

—and increaſing its powers.

It is worthy of notice, that when the vivifying rays of the ſun have cauſed the ſap to flow, theſe grafts inducing the fluid through the pores to every part of the tree, will occaſion in-

* The ſyſtem ſucceeds equally well on pear, as alſo on cherry trees, provided the medication is uſed to prevent the cherry tree from gumming.

numerable

numerable ſuckers or ſcions to ſtart through the bark, which, together with the grafts, give ſuch energy to vegetation, that in the courſe of the ſummer the tree will be actually covered over by a thick foliage, which enforces and quickens the due circulation of ſap. Theſe, when combined, fully compel the roots to work for the general benefit of the tree.

In theſe experiments I judged it proper to make choice of grafts from the ſorts of fruits which were the moſt luxuriant in their growth, or any new variety, as deſcribed in the ſeventeenth and eighteenth volumes of the Society's Tranſactions, by which means a greater vigour was excited; and if this obſervation is attended to, the practitioner will clearly perceive, from the firſt year's growth, that the grafts would ſoon ſtarve the ſuckers which ſhoot forth below them, if they were ſuffered to remain *. With a view to accompliſh this grand object of improvement, I gave much attention, as I have before obſerved, to the general practice of invigorating old trees; and I happily diſcovered the error of the common mode of engrafting but a ſhort diſtance from the trunk or body, as in Fig. 1. Pl. VIII. There the circumference of the wounds is as large as to require ſeveral grafts which cannot firmly unite and claſp over the ſtumps, and conſequently theſe wounds lay a foundation for after-decay. If that were not the caſe, yet it ſo reduces the ſize of the tree, that it could not recover its former ſtate in many years, and it is dubious if it ever would; whereas, by the method of extreme grafting, as *Fig.* 3, the tree will be larger, in three or four years, than before the operation was performed. For all the large branches remaining, the tree has nothing to make but fruit-bearing wood; and from the beautiful verdure it ſoon acquires, and the ſymmetry of the tree, no argument is neceſſary to enforce the practice.

The moſt luxuriant fruits preferred.

Fig. 2 was my firſt experiment about eight years ſince. The error of No. I was there a little amended, and gave me the idea of engrafting at the extremity. Permit me to remark, that thoſe done in my orchards, on the plan of *Fig.* 2, did not, neither were they able to bear ſo many apples laſt ſeaſon, which was a bearing year, as thoſe on the plan of *Fig.* 3,

* This thought ſhould be kept in ſuſpence, as ten years hence it may appear otherwiſe. However, they will be valuable trees, and highly profitable, as will any other brought under the ſame ſyſtem.

which

which produced me about two buſhels each tree of the fineſt fruit I had in my orchards, from the third ſummer's wood only. Some engrafted with Ribſton pippins were beautiful.

Approbation of Mr. Bucknall.

Mr. Bucknall viſited me this ſummer for the expreſs purpoſe of ſeeing my trees; and he ſays the manner of conducting the ſyſtem is the happieſt that ever was conceived. For when a tree has done its beſt, and has continued to extreme oldage, juſt diſpoſed to fall into diſſolution, as alſo when this is the caſe with trees in a ſtagnated and barren ſtate, they are thus renovated, and may, with the greateſt probability, continue valuable for fifty years to come. I need not ſay, do not make the attempt when the energy of growth is over; that will eaſily be ſeen by the body and arms, but more particularly from the ſize, figure, ſhape, and colour of the leaves, which give the proper indication of health or decay in vegetation.

Should the Society deſire it, ſeveral gentlemen reſident here, will gladly ſend up certificates to confirm the ſtatements.

I remain, SIR,

Your moſt obedient ſervant,

W. FAIRMAN.

Millers-Houſe near Sittingbourn, Kent,
Feb. 9, 1802.

CEMENT FOR ENGRAFTING.

Cement for engrafting.

One pound of pitch One do. . . . roſin Half do. . . . beeſwax Qtr. do. . . . hogſlard Qtr. do. . . . turpentine	}	To be boiled up together, but not to be uſed till you can bear your finger in it.

SIR,

Teſtimonial of S. D. Bucknall, Eſq.

THIS is to certify to the Society for the Encouragement of Arts, &c. that William Fairman, of Millers-Houſe, Lynſted, Eſq. has long been a ſteady and zealous promoter of the improvement of the ſtandard fruits of the country; and that he planted one entire orchard, of ſixteen acres, ten years ago.

The ſyſtem of extreme-branch grafting, now introduced to the public, he has had in contemplation full eight years, though not in its preſent ſtyle of ſucceſs and elegance; for he has been improving. In thoſe operated upon within the laſt three

three or four years he has been wonderfully ſucceſsful, and I am happy in an opportunity of adding my teſtimony to the advantages reſulting from this method of renovating old fruit-trees.

An idea equal to the preſent ſyſtem could not have fallen into better hands than thoſe of Mr. Fairman. He is bleſſed with a good ſoil, cultivates the land well, and ſteadily attends to improvement. The gentlemen of the committee, by looking at the three little ſketches of drawings which repreſent the three trees, will ſee that *Fig.* 1 is ſo amputated, as not likely to continue in health, ſo as again to form a good tree; and that *Fig.* 2 will be many years before, if *ever* it does. But there are now many fine large trees in the ſtate of *Fig.* 3, which have been engrafted but three or four years, and yet, as far as ſtructure goes, are complete already, and in two years much fine fruit may be expected.

The ſyſtem is as follows: Make the trees perfectly clean, and keep them as uniformly large as is convenient.

In autumn, 1801, I ſpent ſome days at Lynſted, and ſeveral times walked over the plantations with Mr. Fairman, and was very much pleaſed with their appearance.

I remain, SIR,

Your obedient Servant,

THOMAS SKIP DYOT BUCKNALL.

February 22, 1802.

Reference to the Engraving of Mr. FAIRMAN's *Method of Extreme-Branch Grafting; Plate* VIII. *Fig.* 1, 2, 3, 4.

Reference to the engravings.

Figure 1. diſplays the old practice, commonly called cleft-grafting.

Fig. 2. Improved experiment on *Fig.* 1, by engrafting higher up the tree.

Fig. 3. Shows the method of extreme-branch grafting, recommended from experience, by Mr. Fairman. Two grafts or ſcions are there placed at the extremity of each branch; beſides which, additional grafts are inſerted in the ſides of the branches; as, at AAAAAA, or where they are wanted to form the tree into a handſome ſhape.

Fig. 4. Shows upon a larger ſcale than the former figures the method of applying the grafts at the extremity of the branches, and retaining them by the baſs-mat bandage and cement.

XIV.

Observations on several Pharmaceutical Preparations, by CIT. STEINACHER, *Druggist at Paris. Abridged by* CITIZEN PARMENTIER*.

Unguentum Nutritum.

Unguentum nutritum.

CITIZEN Dubree, an eminent druggist at Rouen, has lately presented a formula for unguentum nutritum, to the Pharmaceutical Society. As apothecaries zealous for the perfection of their art, have proposed improvements in the preparation of this ointment at different periods, I have thought that an object to which the attention of practitioners has been called from time to time, notwithstanding it is apparently obsolete, deserved a fresh examination †.

The mixture of oil, vinegar, and litharge. Requires carbonic acid, to convert the litharge into carbonate, and thicken the oil.

Too much vinegar will prevent the combination.

The formula of the French pharmacopœia the best.

When oil, vinegar, and litharge are to be mixed together into a homogeneous mass, a little litharge must be dissolved in acetous acid ‡, and a sufficient quantity of carbonic acid must be introduced, 1st. to convert the greater part of the litharge into white carbonate, which remains diffused through the oil; 2dly, to thicken the oleous mixture, an effect analogous to the thickening of soups by the carbonic acid, with which we were made acquainted by Pelletier. If a sufficient quantity of vinegar to form a saturated saline compound be employed, the mixture will never combine perfectly. This theory, founded on experiment, brings us back to the prescribed formula, as the best that can be adopted, that which produces an ointment, the most bulky, the lightest, and the most cooling to the part

* *Annales de Chimie*, XLVII. 97. (No. 139.)

† Dr. Aikin, the learned editor of Lewis's Materia Medica, says; "The *unguentum nutritum*, made without heat, though now expunged from our dispensatories, is much the best of the ointments prepared from lead, and a very excellent application in many cases. It should not be long kept, but made fresh as wanted." H.

Litharge completely soluble in acetous acid, but not in common vinegar. The residuum of the latter.

‡ Experience has taught me, that levigated litharge is completely soluble in a sufficient quantity of acetous acid, but that the final residuum of its solution in common vinegar, which has been supposed to be superoxided lead, contains only tartrite and malat of lead, with a great deal of extractive matter, which form a paste with a remnant of the litharge reduced toward the metallic state.

affected.

affected. It ſucceeds very well, when the operator is endued with patience, and works in a cold place. It may be abridged however, if, according to the excellent advice of Baumé, we employ coagulated oil of olives, by which the ſurfaces of contact are increaſed, and the introduction of the air is facilitated. One important fact with reſpect to keeping the preparation is, that at the temperature of 15° or 16°, at which moſt kinds of fermentation take place, a portion of the carbonic acid is extricated, and leaves expoſed an oxide at $\frac{9}{100}$, which becomes again yellow. It requires a temperature of 10° to preſerve its white colour unaltered.

The proceſs may be ſhortened by employing coagulated oil.

Warmth ſpoils it.

But it will keep well in the cold.

Citizen Dubree, and Citizen Granet before him, propoſed to expedite the preparation by adding hog's lard; but I find, that this addition diminiſhes its bulk and levity. In Germany different compoſitions are made under the name of *nutritum*, as with vinegar of litharge and half its weight of oil of roſes, which produce an ointment as white as wax, and of the conſiſtence of a liniment; with vinegar of litharge two parts, and olive oil three parts, which yield a whitiſh ointment of a moderate conſiſtence; with two parts of olive oil, one part of wax, and two parts of vinegar of litharge, which furniſh an ointment of a firm conſiſtence, and a waxy whiteneſs. But all theſe compoſitions are ſimple mixtures, feebly united, by no means reſembling the nutritum of the French ſhops, and not requiring for their formation a mutual reaction between the different particles of the ingredients.

The addition of hog's lard injurious.

Different compounds called nutritum in Germany.

All theſe ſimply mixtures.

Cryſtallization of Phoſphoric Acid.

It is known, that the affinity of phoſphoric acid for water overpowers its force of cryſtallization; in fact this ſaliſiant ſubſtance appears commonly in the form of a thick oil. I have lately obſerved, however, that time, the grand producer of regular cryſtallizations, effects a ſymmetrical combination between its particles.

Phoſphoric acid commonly a thick fluid; yet cryſtallizable by time.

I had prepared half a kilogramme of phoſphoric acid, according to the method of Lavoiſier, with phoſphorus and nitric acid, both of them extremely pure. This acid, freed from nitrous gas, reduced to the conſiſtence of a thick ſyrup, and introduced into a phial with a glaſs ſtopper, had been uſed at different times in the courſe of a year, without exhibiting any peculiar appearance. The year following I let it remain perfectly

Cryſtals formed in it by repoſe.

These cryſtals are priſmatic in thin ſhining laminæ.

fectly at reſt in the phial, which was half full, and cloſely ſtopped. After this period I found the ſurface of the fluid covered with a ſaline cruſt, from which ſhot downward priſmatic cryſtals in ſhining laminæ, an inch long, and a line broad, diverging from a centre. I will not deſcribe their geometrical ſtructure, for they are extremely thin, and embedded in a fluid too viſcous for me to take them out without breaking. Beſides, they are ſtill increaſing; laminæ riſe from the bottom of the veſſel, which touch the ſurface of the glaſs, and ſeem preparing to intermix with the ramifications that ſhoot down from the upper ſtratum. The ſides of the veſſel are the ſeat of this beautiful cryſtallization. The centre remains in part concrete or fluid, whence it follows, that if a very regular diſſipation of the particles of the liquid acid of phoſphorus be occaſioned by repoſe, the ſides of the veſſel contribute to it in great meaſure by affording fixed points, to which the poſitions of affinity moſt favourable to cryſtallization direct themſelves.

Purity of Phoſphorus.

Charcoal combines with phoſphorus during the diſtillation.

Prouſt has informed the public, that, in the diſtillation of phoſphorus, a combination of this ſubſtance with the charcoal conſtantly took place. This important diſcovery extends much farther than its celebrated author has ſhewn.

Phoſphorus purified in Woulfe's method.

Muſſin-Puſchkin's,

or Juch's, ſtill ſhews marks of it.

Take the moſt brilliant and moſt tranſparent phoſphorus, which has not only been ſtrained through chamois leather, according to Woulfe's method, but has alſo been diſſolved ſeveral times in nitro-muriatic acid, as done by Count Muſſin-Puſchkin, or which has been treated with oxigenated muriatic acid, after the mode of Mr. Juch of Wurkburg; let it be heated gently in a long ſlender tube; red parts will ſeparate from it. Put a few grains of this phoſphorus, which is conceived to be ſo pure, on a ſilver ſpoon, and ſet fire to it; a red trace will remain. If the ſpoon be heated in the dark, the red trace will be ſeen ſtill to burn, and a coal will remain impregnated with phoſphoric acid.

Heating with cauſtic alkali incapable of proving the purity of phoſphorus.

Heated oxigenated muriatic.

Mr. Juch has aſſerted, that his phoſphorus is extremely pure, becauſe it no longer becomes black when heated with cauſtic alkali; but it is in fact becauſe the phoſphure of carbone is unalterable by cauſtic potaſh. According to the indiſputable authority of Prouſt, this re-agent is incapable of proving the purity of phoſphorus. I confeſs, that heated oxigenated muriatic

muriatic acid deſtroys part of the carbone of phoſphorus, because the combuſtible power of its oxigen increaſes in the ratio of its elaſticity; but it produces this effect only by burning a proportionate quantity of phoſphorus. On the contrary, when it is cold, and its oxigen is reduced to its natural degree of elaſticity, it is far from deſtroying the carbone, it ſeparates it in the ſtate of black oxide, and converts the phoſphorus into white oxide, while at the ſame time, itſelf returns to the ſtate of ſimple muriatic acid. I have obſerved this fact on a ſtick of tranſparent phoſphorus, which I kept two years in a bottle filled with pure oxigenated muriatic acid, ſaturated at the temperature of 10°. It is impoſſible, therefore, to free the phoſphorus entirely of charcoal. They oxide, or are acidified nearly in proportional quantities; and though the proportion of charcoal may be diminiſhed, the phoſphorus always retains ſome by its power as a whole. In fine, I am obliged to contradict the aſſertion of an illuſtrious maſter, Citizen Fourcroy, "that we are unacquainted with any direct combination between carbone and phoſphorus, though it probably exiſts," and to conſider that product on which chemiſts have hitherto beſtowed the name of pure phoſphorus, as a kind of gangue, from which the radical phoſphorus is diſengaged to enter into a number of combinations, without our being capable of obtaining it in its primitive form.

acid deſtroys part of the carbone.

Cold ſeparates it in the ſtate of black oxide.

Impoſſible to free phoſ horus from charcoal completely.

Miſtake of Fourcroy,

Pure phoſphorus yet unknown.

White Oxide of Phoſphorus.

When phoſphorus is heated in a very long and very ſlender glaſs tube, in a ſand-heat of 100° of the decimal thermometer, it is covered with a mild light, and exhales a white vapour, which condenſes in the upper part of the tube, while, at the ſame time, part of the phoſphorus, with exceſs of carbone, ſeparates with its red colour. This white vapour, which has acquired for its formation a ſlight combuſtion, is a white oxide of phoſphorus at a minimum. The following are ſome of its properties. It is flocculent, poſſeſſed of coheſion, and occupies four times the ſpace of the phoſphorus employed in the experiment. When it is dry, it does not redden litmus paper. It contains caloric, and inflames on the contact of combuſtible ſubſtances. It powerfully attracts the moiſture of the air, and is rapidly converted into phoſphorus acid. It differs greatly from the white oxide of phoſphorus made by the

Mode of con vert ng phoſphorus into a white oxide at a minimum.

Some of its properties.

White oxide of phoſphorus a maximum.

Its properties.

the long action of water, or cold oxigenated muriatic acid. This appears friable and pulverulent. It has loſt almoſt all its latent heat. It is very little inflammable, and does not attract the moiſture of the air. It is acidifiable only by the intimate action of an oxigen that contains caloric highly condenſed, as that of the nitric acid. In a word, it is phoſphorus at a maximum of oxidation.

Regular Cryſtallization of Eſſential Oil of Roſes.

Regular cryſtallization of oil of roſes.

Citizen Steinacher has lately obſerved this with attention. He mixed eight kilogrammes of the magna of damaſk roſes (*roſes pâles*) with ſome parts of water, according to the proceſs of Cit. Demachy; and after a day's maceration he drew off by diſtillation ſixteen kilogrammes of water. This was immediately poured into a large glaſs jar, which was covered with a cloth, and left at reſt. In twenty-four hours he found the ſurface of the water covered with an irideſcent pellicle, interſperſed with little hexhaedrons, very much reſembling the cryſtals of ſnow, which the illuſtrious Cit. Mongé has deſcribed. He informs us, that a ſlight ſhake is ſufficient to tear the cryſtalline gauze, and reduce it to that irregular form of whitiſh ſcales or laminæ, which the oil of roſes commonly aſſumes.

Reſembles the cryſtals of ſnow,

and requires abſolute repoſe.

SCIENTIFIC NEWS.

Extract of a Letter from Dr. Schaub *to* Mr. Parkinson, *dated Caſſel, July* 2, 1803.

I AM buſily employed in the analyſis of various minerals, the reſults of which I ſhall communicate to you in my next. I have noticed among other things alſo, that the metal called tungſten (Wolfram by the Germans) can only be obtained at the higheſt degree of de-oxidation, and that this metal does not belong to the claſs of acidifiable metals; for tungſten cannot be oxidized by means of common proceſſes of oxidation.

Tungſten not acidifiable.

New method of obtaining pure pruſſic acid.

I have diſcovered a new method of obtaining pruſſic acid, in a ſtate of abſolute purity. This proceſs conſiſts in pouring upon one part of pruſſian blue, half a part of ſulphuric acid, diluted

with an equal quantity of water, and ſubſequent diſtillation. The pruſſic acid paſſes over in the alcohol; its odour greatly reſembles the water of the lauro ceraſus.

It is a deadly poiſon to animals. Perhaps theſe notices may intereſt the London chemiſts, &c. &c.

Annotation by the Tranſlator.

Richter's method of obtaining tungſten.

The following method of obtaining tungſten, I believe has not been made known in this country. It is recommended by Richter * a German chemiſt.—F. A.

Let equal parts of tungſten oxide (tungſtic acid) and dried blood be expoſed for ſome time to a red-heat in a crucible; paſs the black powder which is formed into another ſmaller crucible, and expoſe it again to a violent heat in a forge, for at leaſt an hour. Tungſten will then be found, according to this chemiſt, in its metallic ſtate in the crucible.

Meteoric Stones.

Extraordinary meteor and ſhower of ſtones in France.

C. BIOT, member of the National Inſtitute, in a letter to the French Miniſter of the Interior, dated July 20, 1803, gives a detailed account of his inquiries, &c. reſpecting a fire ball which exploded in the neighbourhood of Laigle. The memoir will be ſeparately printed.

On Tueſday, April 26, 1802, about one in the afternoon, the weather being ſerene, there was obſerved from Caen, Pont-Audemer, and the environs of Alencon, Falaiſe, and Verneuil, a fiery globe of a very brilliant ſplendour, which moved in the atmoſphere with great rapidity.

Some moments after there was heard at Laigle, and in the environs of that city in the extent of more than thirty leagues in every direction, a violent exploſion, which laſted five or ſix minutes.

At firſt there were three or four reports like thoſe of a cannon, followed by a kind of diſcharge which reſembled a firing of muſketry; after which there was heard a dreadful rumbling like the beating of a drum. The air was calm and the ſky ſerene, except a few clouds, ſuch as are frequently obſerved.

* Richter ueber die neuen gegenſtande der Chimie. Part I. p. 49.

The

The noiſe proceeded from a ſmall cloud which had a rectangular form, the largeſt ſide being in a direction from eaſt to weſt. It appeared motionleſs all the time that the phenomenon laſted. But the vapour of which it was compoſed was projected momentarily from the different ſides by the effect of the ſucceſſive exploſions. This cloud was about half a league to the north-north-eaſt of the town of Laigle; it was at a great elevation in the atmoſphere, for the inhabitants of two hamlets a league diſtant from each other ſaw it at the ſame time above their heads. In the whole canton over which this cloud hovered, a hiſſing noiſe like that of a ſtone diſcharged from a ſling was heard, and a multitude of mineral maſſes, exactly ſimilar to thoſe diſtinguiſhed by the name of *meteoric ſtones*, were ſeen to fall at the ſame time.

The diſtrict in which the ſtones fell forms an elliptical extent of about two leagues and a half in length and nearly one in breadth, the greateſt dimenſion being in a direction from ſouth-eaſt to north-weſt, forming a declination of about 22°. This direction which the meteor muſt have followed is exactly that of the magnetic meridian; which is a remarkable reſult.

The largeſt of theſe ſtones fell at the ſouth-eaſt extremity of the large axis of the ellipſe; the middle-ſized ones fell in the centre, and the ſmalleſt at the other extremity. It thereby appears that the largeſt fell firſt, as might naturally be ſuppoſed.

The largeſt of all thoſe which fell weigh 17½ pounds. The ſmalleſt he ſaw weighed about two gros, which is the thouſandth part of the former. The number that fell is certainly *above* two or three thouſand. They were friable ſome days after their fall, and ſmelled ſtrongly of ſulphur. Their preſent hardneſs was acquired gradually.

Abſtract of a Memoir on the Febrifuge Principle of Cinchona, by Cit. Seguin *.

THE object propoſed to himſelf by the author in the taſk he undertook was, to point out the means of knowing with certainty the true febrifuge principle of cinchona, to diſtinguiſh the ſpecies that contain it from thoſe that do not, and laſtly to appreciate its quantity and quality.

* Bulletin des Sciences. No. 77.

Hitherto

Hitherto the ſight and taſte have been the only teſts of the preſumable qualities of the peruvian bark of the ſhops; but as theſe have no preciſe ſtandard, and are inapplicable to powdered bark, they very imperfectly indicate the preſence of the febrifuge principle. It was of importance, therefore, to ſubſtitute to theſe means, little better than illuſory, others not only capable of calculation, but likewiſe invariable. Chemical re-agents alone can anſwer theſe ends.

Sight and taſte inſufficient to judge of the goodneſs of bark.

In conſequence Cit. Seguin began by iſolating the reſpective properties of all medicinal ſubſtances, and he examined the action they exert on all other chemical ſubſtances.

Theſe reſearches led him to develope very deciſive characteriſtics in the febrifuge principle of cinchona; which place it in a perfectly diſtinct claſs. The following are its characters.

The febrifuge principle of bark very diſtinct.

It precipitates the ſolution of tan, but not the ſolutions of gelatine and ſulphate of iron.

Its characters.

When cinchona has not all theſe characters, it is a proof that it is mixed with ſomething elſe, or that it does not contain the febrifuge principle.

The author has ſubjected to this analyſis all the known ſpecies of cinchona, found among all the druggiſts and apothecaries of Paris and Verſailles, and conſtantly obtained the ſame reſults.

Various ſpecimens of bark ſubjected to this teſt,

Unfortunately theſe reſearches have ſhown, that but an infinitely ſmall quantity of good, unmixed cinchona, is to be procured in the ſhops; the greater part being either deſtitute of the febrifuge principle, or mixed, or of a very inferior quality, though containing no mixture.

which proved that very little good is to be found in the ſhops.

Theſe reſults are of ſo much the greater importance, becauſe the effects of different kinds of cinchona in fevers are only in proportion to the greater or leſs quantity of the febrifuge principle they contain; and thoſe which contain none, as well as all the ſubſtances that may be mixed with them, are more or leſs injurious to the ſyſtem.

Efficacy of cinchona proportionate to the quantity of febrifuge principle it contains.

The experiments of Cit. Seguin on the febrifuge principle of cinchona, having convinced him that moſt of the bark found in the ſhops was injurious or inefficacious, becauſe it was ſpoiled by keeping, adulterated by mixture, or deprived of the febrifuge principle; he has endeavoured to obtain a febrifuge principle always the ſame, more efficacious, more certain in its effects, more capable of aſſimilation with our ſyſtem, and ſo cheap, that there could be no temptation to adulterate it.

Defects of the common bark of the ſhops.

To attain this important object, the author has inquired what the true cauſe of fevers, as of their effects, is; what the nature of the febrifuge principle of chinchoa, and what its action on our ſyſtem. He has ſubjected to the action of the re-agents pointed out for the febriſuge principle of cinchona, all chemical and medicinal ſubſtances; and aſſured himſelf, whether ſuch of theſe ſubſtances, as might contain the febriſuge principle, did not contain, at the ſame time, other ſubſtances prejudicial to the animal economy. Laſtly, he had to cure fevers by the help of theſe remedies, and then confirm this theory by repeated experiments. Such is the courſe Cit. Seguin has purſued.

The febrifuge principle is gelatine.

The new febrifuge principle, which he propoſes to ſubſtitute inſtead of cinchona, becauſe it unites all the advantages of the bark, without any of its inconveniences, is gelatine in its pure ſtate.

Advantages this poſſeſſes over bark.

Conſidered in a medical, economical, and political view, gelatine promiſes much greater advantages than bark, in its application to the cure of fevers. It occaſions no irritation; procures quiet ſleep and gentle perſpiration; keeps the belly open, without producing colic or nauſea: has no unpleaſant flavour; reſtores the ſtrength, and is digeſted even by the weakeſt ſtomach, that would reject the bark as ſoon as adminiſtered.

Diſadvantages of bark.

On the other hand, cinchona irritates the ſyſtem, diſturbs the ſleep, has a diſagreeable taſte, frequently occaſions coſtiveneſs, and is very indigeſtible.

Comparative cheapneſs of gelatine.

In an economical view, there is ſtill greater difference between cinchona and gelatine; the price of the latter being to that of the former at moſt as one to thirty-two.

Laſtly, gelatine is indigenous, cinchona is not; and the purchaſe of the latter requires us to ſend abroad a very conſiderable ſum of money, which might be kept at home by adopting the uſe of gelatine.

Caſes cured by it.

To this memoir the author has ſubjoined thirty-ſeven caſes, in which he performed a cure with gelatine, under the eyes of ſome reſpectable phyſicians, and he has deſired a Committee to be appointed, to repeat his experiments, and report upon them.

Committee appointed to examine its effects,

Accordingly Citizen Portal, Deſeſſarts, Hallé, Fourcroy, Berthollet, and Deyeux, have been nominated for this purpoſe.

Their experiments are made at the School of Medicine, in a room exclusively appropriated to these inquiries; already a great number of patients have been cured; and the Committee will soon make their first report on these cases. which appear to be confirmed by experience.

Query by a Correspondent respecting the Augustine Earth.

To Mr. NICHOLSON.

SIR,

WE possess many excellent elementary works on Chemistry, both original and translations, such as Thompson, Accum, Murray, Henry, Parkinson, La Grange, Green, Fourcroy, Jacquin, &c. but in none of these authors is mentioned the method for obtaining the new earth, called *Augustine*; although most of these works have been published a considerable time after this earth was made known by the German discoverer, Professor Tromsdorf. I have also made enquiry, concerning this subject, of most of the public teachers of chemistry, and other individuals, who stand high as chemical philosophers, but in vain; I will therefore thank you to allow these lines a place in your valuable Journal. Perhaps one of your Correspondents will be kind enough to favour me with the process for obtaining this earth; for the author of these lines cannot find it in the mineral which is said to contain it; having pursued the usual methods of examining mineral substances for that purpose. Augustine earth.

I am,

SIR,

Your's, &c.

P. O.

Spaniard said to resist high Degrees of Heat and strong chemical Agents.

THE prints of Paris, and some of our own, too implicitly copying them, have for some time exhibited a strange narrative of a young Spaniard, born at Tolosa, and now 23 years of age, of whom it has been very particularly affirmed, Extraordinary story of a Spaniard said to resist heat and corrosive acids.

That

That though his ſkin exhibited no appearance of peculiarity, either natural, or indicating preparation by art, yet without injury, 1. He bathed his feet for ſix minutes, and waſhed his hands and face in oil heated to 250° of Fahrenheit, which is 38 degrees hotter than boiling water. 2. He did the ſame with a ſolution of ſea ſalt, heated 12 degrees higher. 3. He ſtood with his naked feet upon a bar of iron near the welding, or at the white heat; he held the bar in this ſtate in his hands, and rubbed it on the ſurface of his tongue. 5. He waſhed his mouth with the ſtrongeſt ſulphuric and nitric acids, and applied the ſame to the other parts of his ſkin, with no other effect than that the nitric acid produced a yellow tinge; and 6. he remained a conſiderable time in an oven heated to within 18 degrees of the boiling water point.

Though our reaſoning from analogy in matters of experiment, is liable to miſlead, as well by infuſing too much doubt as too much confidence, yet I ſhould have paſſed over this tale without notice, if I had not heard of it from very reſpectable correſpondents. I ſuppoſe there may be ſomething extraordinary in the degree of inſenſibility of the ſubject in queſtion, as the Inſtitute has paid attention to him; but I underſtand that the ſtory is now told with great abatements. Citizen Pinel, a man of information, and well known as an accurate obſerver, is commiſſioned to report upon the ſame; and I have no doubt but his account will ſhew how much eaſier it is for men to tell falſehoods than to reverſe the courſe of nature.

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*Method of giving Malt Spirits the Flavour of Brandy.**

Flavour of malt ſpirits amended.

INTO a quart of malt ſpirits put three ounces and a half of finely powdered charcoal, and four ounces and a half of ground rice. Let theſe ingredients remain during fifteen days, only obſerving to ſtir them often: at the expiration of this time, let the liquor be ſtrained, and it will be found to be much improved.

Preparation of a Lute proper for Chemical Operations. By C. Paysse, *Profeſſor of Chemiſtry* †

Lute, eggs and chalk, or ſlaked lime.

IN the preparation of the oxigenated muriatic acid in the large way, the neceſſity I found for a lute, which, to the ad-

* From the Bibliotheque Phyſique Economique, No. 10. An. XI.

† From the Annales de Chimie, No. 137. An. XI.

vantage

vantage of being cheap, ſhould add thoſe of being eaſily prepared; of reſiſting the action of the deſtructive vapour of the acid, as well as the ſtrong heat which the luted part is often required to bear; which ſhould be eaſy of application, and in an uniform manner, and not harden too quickly; obliged me to make ſome experiments on the ſubject, the reſult of which have been very ſatisfactory.

After making a great number of mixtures with different ſubſtances, I made choice of the following, which produced me a homogeneous compoſition, drying as ſlowly as could be deſired, extremely hard; when dry, of a very compact texture; and, in ſhort, poſſeſſing all the properties I had deſired.

Take the white of two eggs, with their yolks, and of powdered carbonate of lime, or of quick lime well ſlaked in the air, about half the weight of the eggs; ſpread it on a cloth, and apply it as a lute.

NOTE.

THIS lute, the compoſition of which is very ſimple, poſſeſſes a degree of elaſticity, when dry; I have formed veſſels of it, which are impermeable to water, and ſuſceptible of being poliſhed on the wheel. This compoſition reſembles the ſubſtance of which the pipes, called *Meerſhaum*, are made.

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*Two new Quadrupeds.**

Two living Quadrupeds have lately been received at the Muſeum of Natural Hiſtory at Paris, which are entirely unknown among naturaliſts, and were brought to Europe by Captain Baudin. Profeſſor Geoffroy (of Egypt) who has inſerted a deſcription of them in the annals of the Muſeum, calls them Faſcolomes. They come from the weſtern coaſt of New Holland; their fur promiſes to be of ſome uſe; and, according to the opinion of Captain Hamelin and his ſuite, their fleſh is very excellent food. They are particularly intereſting to naturaliſts from the ſingularity of their organization. In the form of the head, the number, arrangement, and nature of the teeth, and the form of the fore feet with which

Two new Quadrupeds from New Holland.

* Decade Philoſ. No. 51. An. XI.

tthey

they burrow in the earth, they reſemble the marmot; but they differ from them, by the female having a pouch beneath the belly, and by the whole ſtructure of the organs of generation, in which they are ſimilar to the *ſarique* of Buffon. The form of the hinder foot is the ſame as in that animal with a pouch; the thumb being diſtinct from the other fingers, and without a nail: the tail is ſo ſhort, that it remains hid below the hair, which is brown, buſhy, and very long. The Faſcolomes of the Menagerie are yet young, but are already larger than rabbits. Their temper is admirable; they may be handled, or removed, without ſhewing any ſymptoms of fear, anger, or uneaſineſs; their movements are heavy and clumſy; they live under ground, ſleep during the day, and go in ſearch of food at night. In general they poſſeſs but little energy or activity; they ſcratch themſelves like the monkey, and they may be fed with bread, milk, roots, and every ſort of herbage.

Preſervation of Iron from Ruſt.

Defence of iron from ruſt.

CIT. Conté has adopted a method, which he finds effectual, for preventing the oxidation of iron and ſteel; or, in popular terms, to prevent iron and ſteel from ruſting. It conſiſts in mixing with fat oil varniſh, at leaſt half, or at moſt four-fifths of its quantity of highly rectified ſpirits of turpentine. This varniſh muſt be lightly and evenly applied with a ſponge; after which the article is left to dry in ſome ſituation not expoſed to duſt. He affirms, that articles thus varniſhed retain their metallic luſtre, and do not contract any ſpots of ruſt. This varniſh may alſo be applied to copper, of which it preſerves the poliſh, and heightens the colour. It may be employed with particular advantage to preſerve philoſophical inſtruments from any change, in experiments where, by being placed in contact with water, they are ſubject to loſe that poliſh and preciſion of form which conſtituted part of their value.

ACCOUNT

ACCOUNT OF NEW BOOKS.

Philoſophical Tranſactions of the Royal Society of London, for the Year 1803. Part I.

THE Contents of this Part are, 1. The Bakerian Lecture, Obſervations on the Quantity of horizontal Refraction; with a Method of meaſuring the Dip at Sea. By William Hyde Wollaſton, M. D. F. R. S. 2. A Chemical Analyſis of ſome Calamines. By James Smithſon, Eſq. F. R. S. 3. Experiments on the Quantity of Gaſes abſorbed by Water at different Temperatures and under different Preſſures. By Mr. William Henry. 4. Experiments and Obſervations on the various Alloys, on the Specific Gravity, and on the comparative Wear of Gold. Being the Subſtance of a Report made to the Right Honourable the Lords of the Committee of the Privy Council, appointed to take into Conſideration the State of the Coin of this Kingdom, and the preſent Eſtabliſhment and Conſtitution of His Majeſty's Mint. By Charles Hatchett, Eſq. F. R. S. 5. Obſervations on the Chemical Nature of the Humour of the Eye. By Richard Chenevix, Eſq. F. R. S. and M. R. I. A. 6. An Account of ſome Stones ſaid to have fallen on the Earth in France, and a Lump of Native Iron ſaid to have fallen in India. By the Right Honourable Charles Greville, F. R. S. 7. Obſervations on the Structure of the Tongue, illuſtrated by Caſes in which a portion of that Organ has been removed by Ligature. By Everard Home, Eſq. F. R. S. 8. Obſervations on the Tranſit of Mercury over the Diſk of the Sun; to which is added an Inveſtigation of the Cauſes which often prevent the proper Action of Mirrors. By William Herſchell, LL. D. F. R. S. &c. 9. An Account of ſome Experiments and Obſervations on the Conſtituent Parts of certain Aſtringent Vegetables; and on their Operation in Tanning. By Humphry Davy, Eſq. Profeſſor of Chemiſtry in the Royal Inſtitution. 10. Appendix to Mr. William Henry's Paper, on the Quantity of Gaſes abſorbed by Water, at different Temperatures, and under different Preſſures.*

APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society, by Order of the Preſident and Council.

* Our Readers will obſerve, that we have as uſual, reprinted in our Journal moſt of theſe valuable Papers.

An

An Eſſay on the Law of Patents for new Inventions; to which are preſixed Two Chapters on the general Hiſtory of Monopolies, and on their Introduction and Progreſs in England, to the Time of the Inter-regnum: with an Appendix containing Copies of the Caveat, Petition, Oath, and other Formulæ, with an arranged Catalogue of all the Patents granted from the 1ſt of January 1800, to the preſent Time. By JOHN DYER COLLIER, 1803. Longman and Rees, Royal 8vo.

One of the moſt obvious expedients for taxing the induſtry of man in ſocial life, but at the ſame time one of the moſt pernicious, conſiſts in monopolies. Accordingly we find in all governments that this reſource is more or leſs adopted, and trades, manufactures, and various operations, become confined to the executive power, or what is worſe, to the private favourites of men of influence. A long ſeries of years have elapſed ſince this nuiſance was aboliſhed in our country, by the ſtatute of James, and the monopolies that yet remain, are under the direct ſanction of law, and ſo few, that a common obſerver would be diſpoſed to ſay we have none.

A claſs of monopolies which has conſtituted the ſubject of a clauſe of exception in that act, conſiſts in the ſole working and making of new manufactures for a limited time under royal grant, to the firſt and true inventor thereof. It has been a ſubject of diſcuſſion whether even this excluſive privilege which is often made the inſtrument of public deception, and ſometimes of oppreſſion by wealthy Individuals to cruſh the induſtry of ingenious men by expenſive legal proceſſes under letters patent, for objects of public poſſeſſion; it has often been diſputed whether this excluſive privilege be a benefit or an evil. The facts I think are, that many private fortunes are loſt, in ſupporting pretended inventors, or in bringing real ones into effect, and that our arts, trade and ſciences are greatly benefitted by this laſt operation:—

The ſubject of patents and monopolies in general is therefore of great intereſt and practical importance, and I have no doubt the public will receive this compendium as a valuable addition to their means of information reſpecting them.

Electrical Calamine.

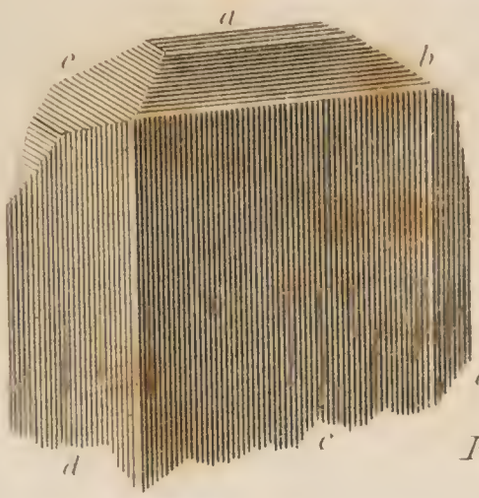
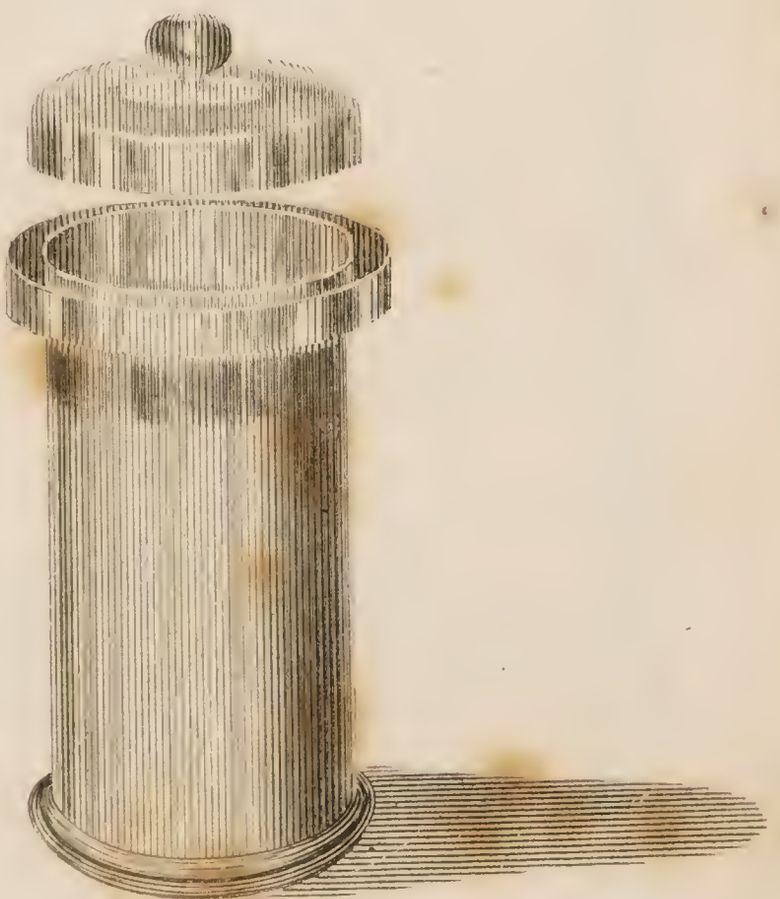


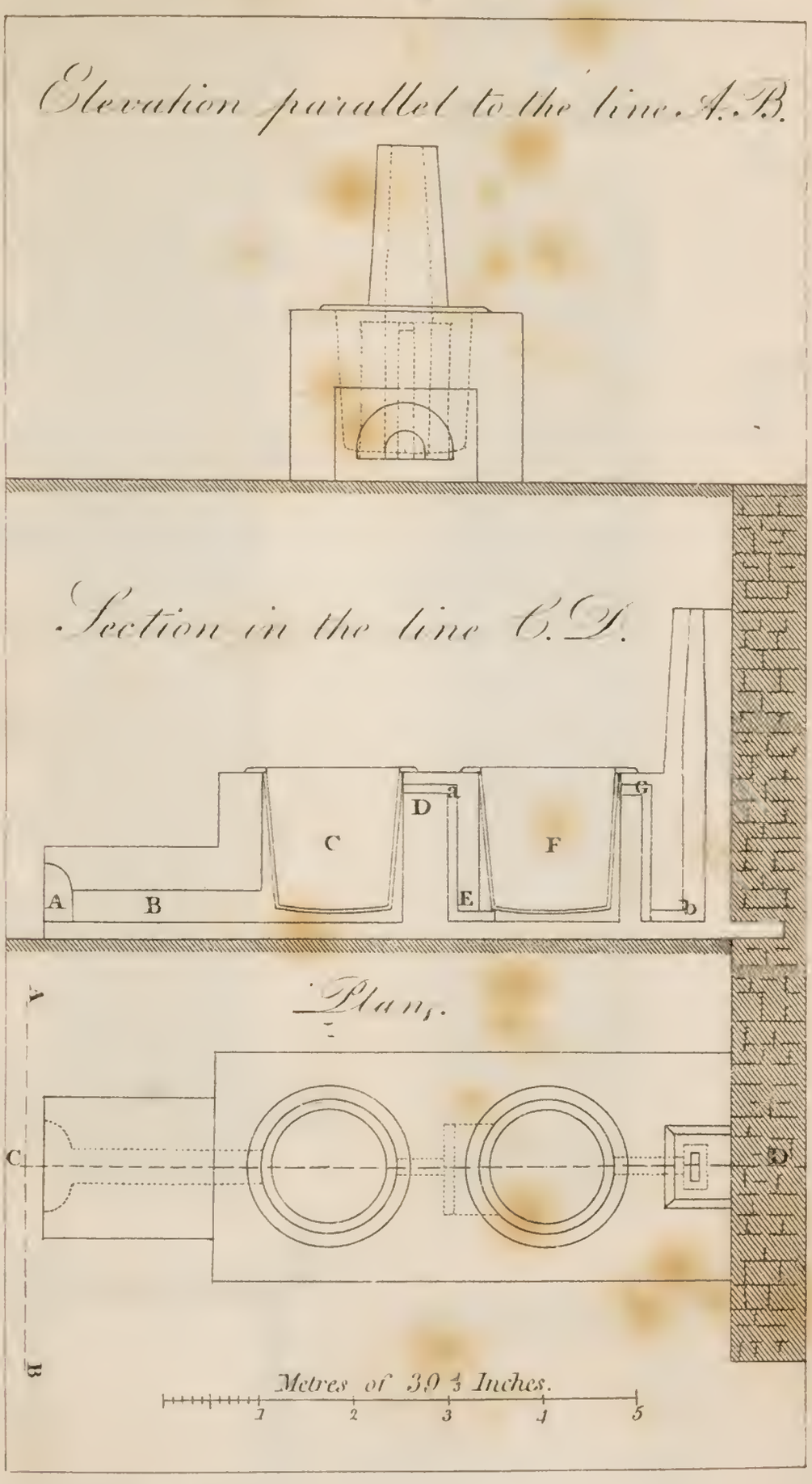
Fig. 1.

Fig. 2.



Closure of Vessels for Anatomical Preparations &c.

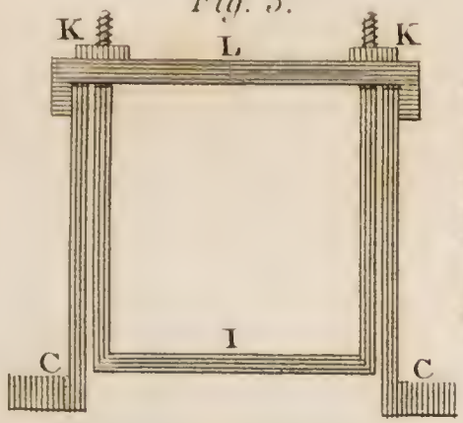
Furnace for Evaporation by regulated Temperature.



Mutlow Sc. Rupzell Co.

M.r Wood's method of supporting Decayed Timbers.

Fig. 3.



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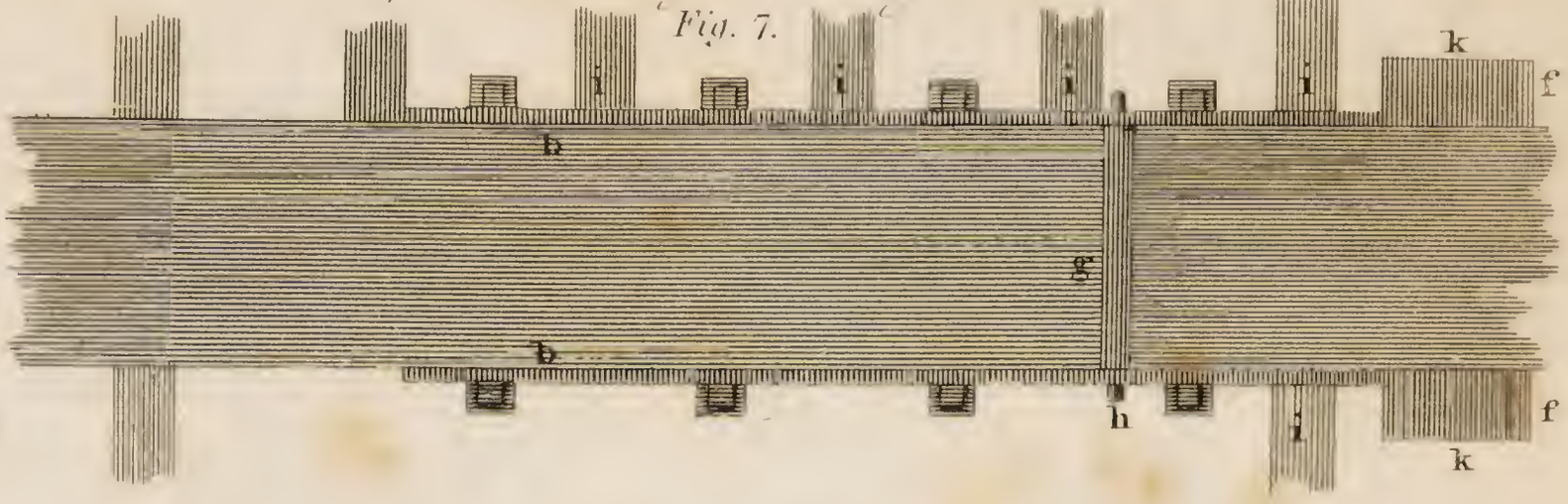


Fig. 4.

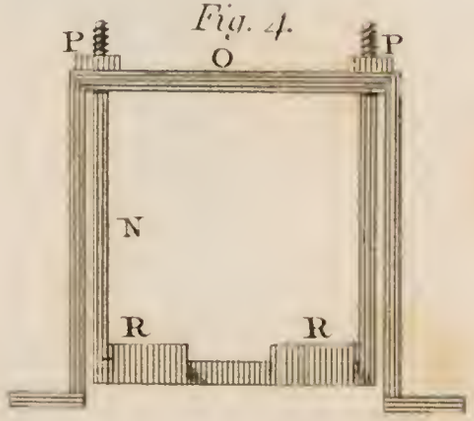


Fig. 6.

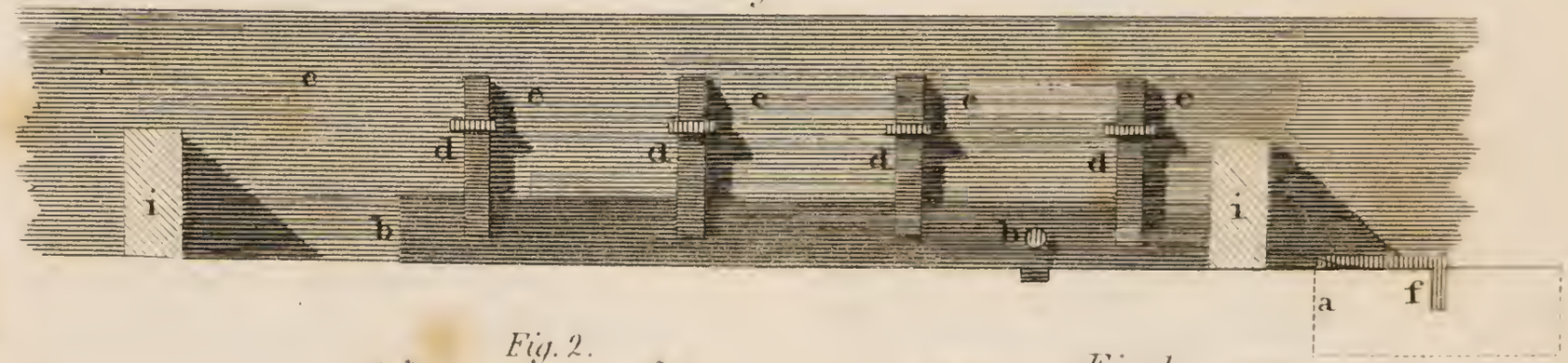


Fig. 5.

Fig. 2.

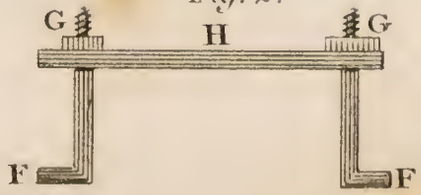
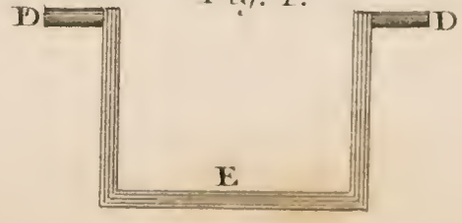


Fig. 1.



Mathew Sc. Bufs. N.Y.

Philos. Journal Vol. VI. Pl. XIX. p. 144.

Mr. Wm. Fairman's Method of Extreme Branch Grafting.

Fig. 1.

Fig. 2.

Fig. 3.

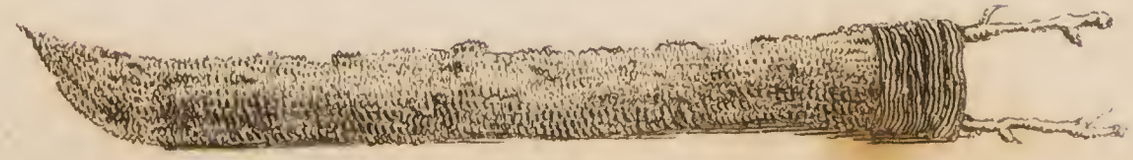


Fig. 4.

Shallow Sc. Rapsell Ct.

Philos. Journal Vol. IV. P. VIII. p. 111.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1803.

ARTICLE I.

Experiments and Obſervations on the various Alloys, on the Specific Gravity, and on the comparative Wear of Gold. Abſtracted from the Memoir of CHARLES HATCHETT, *Eſq.* F. R. S. *in the Philoſ. Tranſ. for* 1803.

(Concluded from Vol. V. Page 303 *of our Journal.)*

Experiment I.

TWELVE pieces of ſtandard gold were firſt examined, and were placed ſo that ſix were oppoſed to ſix. Loſs of ſtandard gold by friction.

The braſs frame, in which each upper piece was fixed, weighed 1604 grains; and it was found neceſſary to add to each a weight of lead, equal to 19825 grains; ſo that the pieces were rubbed againſt each other under the preſſure of 19825+1604=21429 grains = 3 lb. 8 oz. 12 dts. 21 grs *.

* This weight was not employed till repeated trials had proved the extreme difficulty, and almoſt impoſſibility, of producing any perceptible effect with leſs, in a moderate period of time; and, even with this weight, the experiments were found to be exceedingly tedious. The only evil which reſulted from ſuch a preſſure was, that the comparative wear of the fine gold appeared much more conſiderable than would have been the caſe, if a ſmall weight could have been employed; ſome obſervations will therefore be found in the ſubſequent pages, which point out the neceſſity of making an allowance for this circumſtance.

by actual abrafion, as by having the protuberant parts preffed and rubbed into the mafs, in conſequence of its extreme ſoftneſs and ductility.*

2d. That fine gold, or of 23 car. $3\frac{3}{4}$ grs. when rubbed againſt the various kinds of alloyed gold, always or generally ſuffers the greateſt comparative loſs.

Standard gold.

3d. That gold reduced to 22 carats, or to ſtandard, by ſilver, or by ſilver and copper, or merely by copper, ſuffers by friction, under general and ſimilar circumſtances, a ſmaller diminution than the fine gold abovementioned; and, with or without abraſion, the protuberant parts on the ſurfaces of theſe pieces remain much more permanent, under all circumſtances, than thoſe of the fine gold. The difference of wear between the three kinds of ſtandard gold abovementioned, does not in reality appear to be very conſiderable; but, upon the whole, the preference may be given to gold alloyed with a mixture of ſilver and copper, or to that which has only copper for the alloy.

Gold with iron or tin.

4th. That gold made ſtandard partly by the addition of iron or tin, ſuſtains a greater loſs by friction than either of the three kinds of ſtandard gold above-mentioned.

18 carat gold, with copper.

5th. That gold reduced to 18 carats by copper, is more liable occaſionally to wear, in a ſmall degree, than the three kinds of ſtandard gold which have been lately mentioned, provided that the friction takes place between pieces of equal quality; but, in the contrary caſe, the principal loſs always falls on the ſoft or ſtandard gold, when it is oppoſed to gold of 18 carats, which is conſiderably harder.

Gold much debaſed with copper.

6th. That gold more debaſed than that of 18 carats, ſuch as gold alloyed with an equal proportion of copper, ſuffers very conſiderably more than any of the kinds hitherto mentioned, provided that the pieces are of the ſame quality; but, on the contrary, fine and ſtandard gold experience a very great loſs, when expoſed to the action of this debaſed gold, while the loſs of the latter is comparatively much leſs.

Diſadvantage of ſoftneſs in coin.

* This is, however, of much conſequence; for, although coin may not ſuffer by actual abraſion, yet, if the impreſſion made upon it can ſo ſoon be deſtroyed, it follows of courſe, that the pieces become (although ſtill allowed to be current) no better than mere blanks, or fragments of a bar or ingot.

7th

7th. That the wear of ſtandard ſilver appears to be nearly equal with that of fine gold; but more than that of gold made ſtandard by ſilver or by copper, and leſs than that of gold much debaſed by copper. Standard ſilver.

8th. That, as gold which is not inferior to ſtandard wears in general leſs than ſtandard ſilver, ſo does this laſt ſuffer much leſs than copper,

The loſs ſuſtained by copper, when rubbed againſt copper, is infinitely more than that of the former metals; and, when theſe are expoſed to the action of copper, they, as well as the copper, ſuffer a very conſiderable loſs. This appears from the general reſults of theſe experiments, which prove, that pieces of metal which are the moſt ſubject to wear, are thoſe which produce the greateſt loſs upon other pieces of metal, when rubbed againſt them; and it is remarkable, that *in ſuch a caſe*, the loſs does not always fall on one in preference to the other; ſo that the wear can only be conſidered in the aggregate, although one of the pieces may be regarded as the principal cauſe. Copper

In order, however, to illuſtrate the reſults of the preceding experiments, as far as they concern the ſofter and harder kinds of ſtandard gold, and to aſcertain more fully the comparative wear of flat and ſmooth ſurfaces with that of ſuch as were partly protuberant, an experiment was made, with two kinds of ſtandard gold: 1ſt. Gold made ſtandard by fine Swediſh copper, which was very ductile; and, 2d. Gold made ſtandard by a mixture of fine Swediſh copper and dollar copper. This was as brittle as was compatible with rolling and ſtamping; and was prepared by melting gold made ſtandard by fine Swediſh copper, with an equal quantity of gold rendered brittle by the ſtandard proportion of Swediſh dollar copper, which was mentioned in the firſt ſection of this paper. Gold made ſtandard by different kinds of copper.

It may here be obſerved, that a diſtinction muſt be made between hard and brittle metal. If a metal is diſpoſed to crack when rolled, without requiring any extraordinary force to enable it to paſs the rollers, then it may be regarded as brittle; but, if it requires an extraordinary force to make it paſs the rollers, and is not diſpoſed to crack, then it may be conſidered as hard. Diſtinction between hard and brittle metal.

This

This experiment proves,

Very ductile and harder gold compared.

1ſt. That very ductile ſtandard gold, when expoſed to the friction of gold of a ſimilar quality, ſuffers leſs by abraſion than gold which is comparatively brittle, or harder, and which is ſubjected to friction under the ſame circumſtances.

2d. That when ſoft gold and brittle or hard gold rub againſt each other, the greateſt loſs is ſuſtained by the ſoft gold. And,

3d. That pieces which have raiſed or emboſſed ſurfaces, ſuffer a greater loſs, under every circumſtance, than thoſe which are ſmooth and flat.

Coin rubbing againſt coin, in ordinary circulation, loſes but little.

The whole of the foregoing experiments were made with the machine called No. 1; and, as the friction was continued, in each experiment, during many days, with a preſſure upon each couple of pieces equal to 3 lbs. 8 oz. 12 dts. and 21 grs., and as (conſidering the ſeverity of ſuch a trial) the loſs ſuſtained by the pieces, ſeparately or collectively, was not very conſiderable, it may with reaſon be inferred, that ſtandard gold does not eaſily ſuffer abraſion by the friction of metal againſt metal, or of coin againſt coin, eſpecially under the circumſtances which commonly prevail during the circulation of money.

In the machine No. 1, the pieces of gold were oppoſed face to face; it now therefore appeared proper, that the facts thus aſcertained concerning the wear of gold, of different degrees of ductility, ſhould be farther examined, and corroborated by a different method. To effect this, the ſecond of the machines before deſcribed, was employed.

Experiments with machine, No. 2.

Two hundred pieces of gold, of five different qualities, were employed in this experiment; twenty pieces of each kind were plain and ſmooth, the others were ſtamped with the die already mentioned. The two hundred pieces were mingled, and were encloſed within the cubic box.

Different qualities of the gold.

The following were the qualities of the gold. 1. Gold of 23 car. $3\frac{3}{4}$ grs. 2. Gold made ſtandard by ſilver. 3. Gold made ſtandard by ſilver and copper. 4. Gold made ſtandard by fine Swediſh copper. 5. Gold made ſtandard by equal parts of fine Swediſh copper and dollar copper.

Their loſs.

After 71720 revolutions of this machine, performed in 40 hours, the loſs ſuſtained was found to be as follows: of No. 1.

the

the unſtamped pieces 92,8 grs. ſtamped, 95,6 grs. No. 2, unſtamped, 63,5 grs. ſtamped, 60,1 grs. No. 3, unſtamped, 12 grs. ſtamped, 11 7 grs. No 4, unſtamped, 18 grs. ſtamped, 19,2 grs. No. 5, unſtamped, 13 grs. ſtamped 12,1 grs. The total weight of the unſtamped pieces, before friction, was 13701,3 grs. Their total loſs, 199,3: the weight of the ſtamped, 13679,5: the loſs 198,7.

All the pieces appeared to have ſuffered more on the edges than on the faces; and thoſe which were ſtamped had the impreſſion more or leſs obliterated or flattened, in proportion to their reſpective degree of ductility, or to the loſs which according to the reſult of this experiment, they had relatively ſuſtained. Edges worn moſt,

The different pieces, after the experiment, had a curious appearance; for, on the edges, which were become round and poliſhed, a ſmall regular raiſed bead or moulding was formed, which ſurrounded each face, like a frame; and both faces were become more or leſs concave. a raiſed bead round the edge; the faces concave.

The original diameter of the pieces was alſo diminiſhed, nearly according to their different degrees of ductility, and according to the loſs which they had experienced in conſequence of the operation. The diameter diminiſhed.

The meaſure of the diameters of the pieces, after the experiment, was,

Gold 23 car. $3\frac{3}{4}$ grs. eight-tenths of an inch and $\frac{3}{40}$.

Gold alloyed with ſilver, nine-tenths of an inch.

The others varied little from nine-tenths and $\frac{1}{40}$; which was leſs, by about $\frac{1}{40}$ of an inch, than the original diameter of the pieces; and it was evident, that the pieces of fine gold, and thoſe conſiſting of gold alloyed with ſilver, being the moſt ductile, had ſuffered the greateſt loſs, and were moſt diminiſhed in diameter. Upon the whole, therefore, this experiment appears to corroborate what has been aſſerted concerning the former, *viz.* that ſoft or ductile gold ſuffers the greateſt loſs, when expoſed to friction in contact with gold which is comparatively harder. Theſe experiments for aſcertaining the effects ariſing from the friction of coin againſt coin being gone through, another ſeries was commenced with the apparatus, No. 3, by means of which various pieces were expoſed to the action of certain powders and filings of metals, which were ſeparately ſprinkled upon the horizontal table. Apparatus, No. 3.

The

The pieces were properly fixed in their reſpective ſockets and frames, and were placed ſo as to bear upon the table, with or without additional weights.

The table was moved by a wheel and pinion, ſo calculated as to avoid too rapid a motion; and the revolutions were denoted, as in the former experiments, by means of a counter.

Friction of gold by whiting, ſand, filings of ſtandard gold, and of iron.

The table was covered with fine powdered whiting, with fine white writing ſand, with filings of gold made ſtandard by copper, and laſtly filings of iron. The laſt three were fixed on the table by means of a ſolution of iſinglaſs. Gold of different kinds as before, ſtandard ſilver, and fine copper, both ſtamped and unſtamped, were ſubjected to the different trials.

General reſults.

From the whole of the preceding experiments, made with the three different machines, viewed and compared together, the author infers.

1ſt. That when equal friction, aſſiſted by a moderate preſſure, takes place between pieces of coin of a ſimilar quality, abraſion is moſt commonly produced in an inverſe ratio to the ductility.

2d. That the contrary effect happens, when pieces of different qualities rub againſt each other; for then, the more ductile metal is worn by that which is harder.*

3d. That earthy powders and metallic filings produce ſimilar effects, and tend to wear the different kinds of gold in proportion to their reſpective degrees of ductility.

Fine gold loſes its impreſſion.

Fine gold, being extremely ſoft and ductile, ſuſtains a conſiderable loſs, under many of the general circumſtances of friction; and as at all times it appears certain, that the impreſſions which have been ſtamped upon it are moſt eaſily obliterated, even when actual abraſion does not take place, there is much reaſon to conclude, that gold of ſuch extreme ductility is not that which is the moſt proper to be formed into coin.

Very hard gold improper for coin: Why.

But gold of the oppoſite quality, or at leaſt ſo hard as to be juſt capable of being rolled and ſtamped, ſeems to be equally improper for the purpoſe of coin. For, even ſup-

* Some experiments made at Paris, in 1790, upon pure and upon alloyed ſilver are conciſely mentioned, the reſults of which appear to be nearly the ſame as thoſe of the preſent experiments upon gold.

poſing

poſing that hard gold ſuffered, in every caſe, leſs by friction than that which is moderately ductile, (which is not however the fact,) and allowing that ſtandard gold may, by a mixed alloy, be rendered as hard as gold reduced by copper to 18 carats, without changing the ſtandard proportion of gold, yet it would be very difficult always to make ſuch ſtandard gold of an uniform degree of hardneſs. Moreover, by ſome experiments which Mr. H. purpoſely made at the Mint, upon the rolling and ſtamping of gold of different qualities, it evidently appeared, that gold equal in hardneſs to that of 18 carats, could not be employed with advantage; for, the additional labour which was required for the rolling and ſtamping of this hard gold, the frequent failure in making the impreſſion, and the battering and breaking of the dies, fully proved, that the expence and difficulty attending the working of ſuch gold, would by no means be compenſated by any ſmall degree of durability which it might poſſeſs over any other.

Gold of moderate ductility beſt.

The extremes of ductility and of hardneſs being therefore equally objectionable, it follows of courſe, that gold of moderate ductility muſt be that which is the beſt adapted for coin; and, as nothing but ſilver or copper can be employed to alloy gold which is intended to be coined, it may be here obſerved, that whatever might have been the original motive for introducing the preſent ſtandard of 22 carats, yet it appears, from the experiments lately deſcribed, that this proportion of $\frac{1}{12}$ of the above-mentioned metals, is (every circumſtance being conſidered) the beſt, or at leaſt as good as any, which could have been choſen.

There is, however, ſome difference in the quality of gold, when alloyed with the ſtandard proportion of ſilver, of ſilver and copper, and of copper, which requires to be conſidered.

Gold alloyed with ſilver; its advantages;

Gold alloyed with one-twelfth of ſilver, is of a fine but pale yellow; it is very ductile; it is eaſily rolled, and may be ſtamped without being annealed; it conſequently does not require to be blanched; and, after the complete proceſs of coining, the ſurface and every part remains of an uniform quality, ſo that, by wear, it does not appear of different colours.

Theſe properties are certainly much to be valued; but the objections to this kind of ſtandard gold are,

1ſt.

Its diſadvantages;

1ſt. The additional expence attending the uſe of ſilver as an alloy.

2d. The extreme pale yellow colour. And,

3d. That, from its great ductility, it is almoſt as liable to have the impreſſions which have been made upon it obliterated, as thoſe which have been made upon fine gold.

All things being therefore conſidered, gold alloyed only with ſilver, does not appear to be ſo proper for coin as may at firſt be imagined.

with equal parts of ſilver and copper; its advantages;

Gold made ſtandard by a mixture of equal parts of ſilver and copper, is not ſo ſoft as gold alloyed only with ſilver; neither is it ſo pale, for it appears to be leſs removed from the colour of fine gold than either the former or the following metal.

Gold alloyed with ſilver and copper, when annealed, does not become black, but brown; and this colour is more eaſily removed by the blanching liquor, or ſolution of alum, than when the whole of the alloy conſiſts of copper. It may alſo be rolled and ſtamped with great facility; and, under many circumſtances, it appears to ſuffer leſs by friction, than gold alloyed by ſilver, or by copper alone.

its diſadvantages:

But, after it has been ſubjected to the ordinary friction which muſt take place during the circulation of money, it is liable to appear of a deeper colour in thoſe parts which are prominent, and are conſequently the moſt expoſed to friction. This defect ariſes from a cauſe which will ſoon be explained, but it cannot be regarded as an objection of any weight.

alloyed with copper alone.

The laſt kind of ſtandard gold which remains to be mentioned, is that which is alloyed only by copper. This is of a much deeper colour than thoſe which have been hitherto noticed, and it is ſlightly harder than either of them; but nevertheleſs it is very ductile, provided that the copper be pure. It requires to be annealed, and then becomes nearly or quite black: which colour is not ſo eaſily removed by the blanching liquor, as that which is produced by the proceſs of annealing, upon gold alloyed with a mixture of ſilver and copper.

It ſuffers leſs by many of the varieties of friction, than gold which is alloyed with ſilver; but, in ſome caſes, it ſeems to wear rather more than gold alloyed with ſilver and copper; the difference is not however very conſiderable.

This

This ſort of ſtandard gold, as well as that which is alloyed with ſilver and copper, appears commonly, after a certain degree of wear, of a coppery colour, more or leſs deep, in thoſe parts which are the moſt prominent; and, when coin thus alloyed exhibits ſuch an appearance, it is frequently and vulgarly ſaid to have been in contact with copper money; and ſometimes guineas having this appearance have been refuſed, upon the ſuppoſition that they were debaſed. But the real fact is, that when copper conſtitutes part or the whole of the alloy, it becomes oxidized or calcined upon the ſurface of the blanks, by the proceſs of annealing; and the blackiſh cruſt of copper, in this ſtate, muſt then be removed by the ſolution of alum, called the blanching liquor. Now it is evident, that after this operation, the ſurfaces of the blanks or unſtamped pieces, can no longer be regarded as ſtandard gold. For, if copper alone forms the alloy, it muſt be diſſolved and ſeparated from the ſurface of each piece of coin; and the ſame effect muſt alſo take place, with reſpect to the copper, in the alloy formed of copper and ſilver. So that, in the firſt caſe, each piece, when blanched, will conſiſt of gold made ſtandard by copper, covered with a thin coat of fine gold; and, in the ſecond caſe, each piece will be compoſed of gold made ſtandard by ſilver and copper, coated with gold alloyed with $\frac{1}{24}$ of ſilver, or with half of the ſtandard proportion of alloy, ſuppoſing the ſilver and copper to have been in equal quantities. As, therefore, the ſtandard gold of which the pieces conſiſt is always, more or leſs, of a deeper colour than the coating or film of the finer gold which covers each piece, it muſt be evident, that when this coating has been rubbed and removed from the raiſed or prominent parts, theſe will appear of a very different and deeper colour than the flat part or ground of the coin. The reaſon therefore is ſufficiently apparent, why gold which is alloyed with ſilver only, cannot be liable to this blemiſh.

Upon a compariſon of the different qualities of the three kinds of ſtandard gold which have been lately mentioned, it appears, (ſtrictly ſpeaking,) that gold made ſtandard by ſilver and copper is rather to be preferred for coin; but, as gold made ſtandard by copper alone is not very much inferior in its general properties, it may be queſtioned, whether the few advantages which are thus gained, will compenſate

Compariſon of the three kinds.

penſate the additional expence of the ſilver required for half of the alloy; and, indeed, any extraordinary addition of ſilver appears to be the leſs neceſſary, as there is commonly ſome ſilver in the gold which is ſent to the Mint, which, being reckoned as part of the alloy, contributes to produce thoſe beneficial effects which reſult when ſilver is purpoſely added.

From a general view of the preſent experiments, there does not appear to be any very great or remarkable difference in the comparative wear of the three kinds of ſtandard gold, all of which ſuffer abraſion ſlowly, and with much difficulty; and (as it has been already obſerved) the difference of wear between the two laſt mentioned, is certainly but inconſiderable. For theſe reaſons, and from the conſideration of every other circumſtance, it muſt be evident, that the extraordinary loſs which the gold coin of this kingdom is ſtated to have ſuſtained within a certain limited time, cannot, with even a ſhadow of probability, be attributed to any important defect in the compoſition or quality of the ſtandard gold; and all that can be ſaid upon this ſubject is, that ſome portion of this loſs may have been cauſed by the rough impreſſion and milled edge now in uſe, by which each piece of coin acts, and is acted upon by the others, in the manner of a file.

Extraordinary loſs ſuſtained by our gold coin not imputable to fair wear.

The loſs thus occaſioned cannot however be conſiderable; for the quality of the preſent ſtandard gold is certainly that which is well adapted to reſiſt abraſion, eſpecially in the caſe of the friction of coin againſt coin; and this is ſtrongly corroborated by the obſervations of bankers and others, who are in the habit of ſending or receiving large quantities of gold coin from any conſiderable diſtance. When a number of guineas, rather looſely packed, have been long ſhaken together by the motion of a coach or other carriage, the effects of friction are obſerved chiefly to fall upon only a few of the pieces, But it is not a little remarkable, that although theſe are often reduced nearly or quite to the ſtate of plain pieces of metal or blanks, yet, upon being weighed, they are found to have ſuſtained little or no loſs; and from this it appears, that the impreſſions have been obliterated, not by an actual abraſion of the metal, but by the depreſſion of the prominent parts, which have been forced into the maſs, and become reduced to a level with the ground of the coin. Pieces of hard gold would not ſo eaſily ſuffer by depreſ-

ſion; but the real loſs would probably be greater, they being, in the caſe of the friction of coin againſt coin of ſimilar quality, more ſuſceptible of abraſion.

Upon the whole, there is every reaſon to believe, that our gold coin ſuffers but little by friction againſt itſelf; and the chief cauſe of natural and fair wear probably ariſes from extraneous and gritty particles, to the action of which the pieces may occaſionally be expoſed in the courſe of circulation. But ſtill it muſt be repeated, that the united effects of every ſpecies of friction to which they may be ſubjected, *fairly and unavoidably*, during circulation, cannot produce any other wear than that which is extremely gradual and ſlow, and ſuch as will by no means account for the great and rapid diminution which has been obſerved in the gold coin of this country.

SECTION II.

ON THE SPECIFIC GRAVITY OF GOLD WHEN ALLOYED BY VARIOUS METALS.

The problem of finding ſpecific gravities is attended with numerous difficulties.

Difficulties in aſcertaining the ſpecific gravities of bodies, particularly from the inaccuracies of balances, the application and temperature of water, and the poroſity of the object itſelf. Metals vary in their denſity by caſting in a mould, by ſpeedy or ſlow cooling, and by hammering.

Hammering.

Hammering and rolling is an imperfect remedy, and not applicable to the brittle metals.

Alloys affect the ſpecific gravity of gold very ſingularly.

The effects of alloys on the ſpecific gravity of gold are very intricate, and only capable of being determined by a direct trial: for a numerous ſeries of experiments clearly proved, not only that the ſpecific gravity of the compound may differ from the mean of the component parts, but that the effect of the ſame alloy, inſtead of being proportionate to the quantity employed, may differ conſiderably from this. To the peculiar effects produced by certain proportions of ſome of the metals muſt be added the effects peculiar to certain compound alloys, whence ariſes an immenſe complicated ſeries of alterations in ſpecific gravity, never yet inveſtigated by philoſophers.

Compound alloys.

Expanſion produced in gold by ſilver;

With regard to the expanſion or contraction of the compound, little alteration appears to be produced by alloying gold with $\frac{1}{12}$ of pure ſilver, as it produced only an expanſion of 0,10. With copper it was 0,66: with equal parts of ſilver and cop-

copper;

per

iron.

Contraction by tin, bismuth, zinc, and antimony.

per the expansion was 0,67; with iron it was 0,44; with iron and copper 0,37. Tin, bismuth, zinc, and antimony, produced a contraction. Lead and bismuth very much resemble each other in their effects on gold, and in the irregularity of these in various proportions.

Mistake of Brisson in the alloy of gold with copper.

Mr. Brisson has observed, that on alloying gold with $\frac{1}{112}$ of copper, a mutual penetration takes place; but in the course of the present experiments the reverse of this was found. It is probable, therefore, that his experiment was made with part of a large bar or ingot; as the unequal diffusion of the alloy, the quantity of the metal, and the nature, form and position of the mould, are all capable of affecting the specific gravity.

Specific gravity of a mixed metal affected by various circumstances. Differences in the same bar. Whence.

Thus, when the mixture is perfect, the bottom of a bar cast in a vertical mould will be of the greatest specific gravity, owing to the pressure of the superincumbent metal, while the quality of both ends appears equal by the assay. On the contrary, if the mixture be imperfect, the lighter metal flowing out of the crucible first, will render the bottom of the bar inferior both in quality and in specific gravity, as was found by experiment.

Unequal diffusion of alloy.

This unequal diffusion of the alloy through the mass, the exact distribution of which is not so easy as may be imagined, particularly in large quantities, is the most frequent cause of the variation in the specific gravity of standard gold. The difficulty has been considered, and an allowance made for it, called the remedy for the master of the mint. Even when metals have been completely mixed, if they be kept in fusion under certain circumstances, a separation, more or less perfect, sometimes takes place. This separation appears to be according to the relative affinities and specific gravities of the two metals, and is the soonest effected when the metals have not been perfectly mixed *.

Mixed metals separate under fusion.

Long continued friction lessens specific gravity of metals. True chemical combinations.

Beside the causes mentioned there is another, that occasions variations more or less considerable in the specific gravity of

* Some compound metals may perhaps be mere mechanical mixtures; but I am inclined to believe, that by much the greatest number are true chemical combinations; and consequently, when these last have been properly formed, a separation of the component metals, by the means above-mentioned, can seldom if ever be effected. *C. H.*

metals,

metals, and does not appear to have been noticed. This is long continued friction, which always produced a diminution in the ſpecific gravity of the pieces of metal expoſed to it.

Among the other leſs powerful cauſes which produce ſome alteration in the ſpecific gravity of gold, the proceſſes of rolling, and of annealing, may alſo be enumerated; for, in the courſe of theſe experiments it appeared that the ſpecific gravity of the bars, &c. was in a ſmall degree increaſed by rolling, and that the contrary effect was produced by annealing.

Rolling increaſes it:

Annealing diminiſhes it.

The ſpecific gravity of gold, 23 car. $3\frac{1}{4}$ grs. fine, when rolled and ſtamped without being annealed, was found to be 19,277; but, when the ſame was annealed, the ſpecific gravity was 19,231, after ſtamping.

Mr. H. is however, inclined to believe, that annealing had reduced the ſpecific gravity to much leſs than is here ſtated; and that the ſubſequent operation of ſtamping had, in ſome meaſure, compenſated the effects of annealing. For, in the experiments lately mentioned, it was proved, that the ſpecific gravity of the pieces which had not been annealed, was reduced, by long continued friction, from 19,277 to 19,171; an effect ſurpaſſing that which reſulted from annealing by ,060 (19,231 − 19,171 =,060;) and, if heat was the cauſe, the reverſe might have been expected, inaſmuch as the annealing heat exceeded that which was produced by friction; but, as this was not the caſe, he is induced to be of opinion, that the ſpecific gravity was again increaſed, by the ſubſequent ſtamping of the annealed pieces.

Cauſes of variation in ſpecific gravity enumerated.

In addition, therefore, to thoſe cauſes of variation in ſpecific gravity which are the immediate conſequences of hydroſtatical operations, ſuch as, the different height of the column of water, and the changes of temperature to which it is expoſed during the experiments, the following, as far as they concern metallic ſubſtances, may be enumerated.

1. Imperfections in the interior of the maſs, which are produced during the proceſſes of melting and caſting.

2. The difference of denſity in parts of the ſame maſs, reſulting from the quality and quantity of the metal, from the nature of the mould, from the more or leſs vertical poſition of it, and from the height of the column or bar of metal which is caſt.

3. The

3. The unequal diſtribution of the metal, or metals, employed as an alloy, throughout the maſs intended to be alloyed.

4. The peculiar effects which certain metals produce, when uſed ſingly or conjointly as alloys, and which are very different from the reſults of calculation.*

5. Heat, whether produced by friction or excited in any other manner.

Abſolute preciſion not to be expected.

As, therefore, the ſpecific gravity of metals is liable to be influenced by ſuch a variety of cauſes, it is almoſt in vain to expect abſolute preciſion in the reſults of experiments made by different perſons; but, at the ſame time, it may be obſerved, that by proper care and attention to the above circumſtances, a degree of accuracy may be attained, ſufficient to anſwer almoſt every uſeful purpoſe, although, from what has been ſaid, it muſt appear improper to form opinions upon ſmall fractional variations. By the experiments which were made, with every poſſible precaution, upon ſeparate and intire ingots of gold, reduced to ſtandard by ſilver, by ſilver and copper, and by copper alone, when caſt in an iron mould like a cupel, it appeared, that the ſpecific gravity of each of theſe kinds of ſtandard gold is as follows.

Specific gravity of ſtandard gold of different kinds.

Gold made ſtandard by ſilver - - - -	17,927
Gold made ſtandard by ſilver and copper -	17,344
Gold made ſtandard by copper - - - -	17,157

That of our ſtandard gold muſt occaſionally differ.

Now, as our gold coin commonly contains ſilver as part of the alloy, and as at different times this proportion of ſilver muſt have been various, and even conſiderable, particularly when the gold of Portugal, which is alloyed with ſilver, was brought to the Mint, it naturally follows that, excluſive of the many other cauſes of variation which have lately been enumerated, the ſpecific gravity of our ſtandard gold muſt occaſionally be different,

* There can be no doubt but that the effects of compound alloys are, in general, very different from thoſe of each metal ſeparately conſidered; and that ſuch metallic combinations or compound alloys, like neutral ſalts, and many other compounds, have peculiar properties, which act variouſly upon the metals to which theſe compound alloys are added. A great number of accurate experiments are, however, requiſite to elucidate a queſtion ſo intricate.

It may here be alſo obſerved, that the peculiar properties of compound alloys, prove them to be real chemical combinations. *C. H.*

according

according to the relative proportions of ſilver and copper which compoſe the alloy ;* and, as the ſpecific gravity of gold made ſtandard by ſilver is, in the ingot caſt under the above circumſtances, 17,927, while that of gold made ſtandard by copper is only 17,157, ſo, according to the relative proportions of theſe two metals, when united in the alloy, the ſpecific gravity of the ſtandard gold may vary between the two extremes of 17,927 and 17,157, although the real quality or value of the ſtandard gold remains unchanged; and indeed, when ſome allowance is alſo made for ſmall variations ariſing from other cauſes, the range of the different ſpecific gravities of gold made ſtandard by ſilver and copper, may be conſidered as nearly extending from 18 to 17.

Specific gravity of fine gold eſtimated too high.

There is much reaſon to believe, that the ſpecific gravity of fine gold has been too highly eſtimated; and hence a notion has been too commonly received in this country, which has injurioufly and unjuſtly been believed on the continent, that the ſtandard gold of the preſent reign is inferior to that of the reigns preceding it. But the real fact is preciſely the reverſe. If a few of the old coins have proved better than ſtandard, they were much inferior in the aggregate.

Miſtaken notions injurious to our coin.

Our old coin not ſo good as the new.

II.

A Memoir on the Appearance of Spectres or Phantoms occaſioned by Diſeaſe, with Pſychological Remarks. Read by NICOLAI *to the Royal Society of Berlin, on the 28th of February,* 1799.

On the philoſophical diviſions reſpecting body and mind.

PHILOSOPHERS divide the human being into body and mind, becauſe the numerous and diſtinct obſervations we make on ourſelves oblige us to conſider man particularly, as well in reſpect to his corporeal as his mental functions. Other philoſophers have ſuppoſed that this ſubject might be treated with greater perſpicuity by conſidering man as compoſed of body, ſoul, and mind. There can be no doubt but that theſe, and even more diviſions might be invented. Such philoſophers,

* The firſt guineas which were coined, or thoſe of CHARLES II. and JAMES II. were generally alloyed with ſtandard ſilver; but the coins of the ſubſequent reigns have been alloyed with copper, added to compenſate the deficiency of alloy, or of ſilver in the gold.

however, have by no means conſidered that arbitrary ſyſtematic diviſions, do not conſtitute an inveſtigation of nature, and that philoſophy often becomes more uncertain the more preciſely we endeavour to diſtinguiſh and ſeparate what nature has cloſely united. Sub-diviſions in ſpeculation ſeem as neceſſary as fences in fields, both are in themſelves unproductive, and the more they are multiplied and extended the greater is the diminution of the fertility.

They are of little uſe,

For my part, I will confeſs, that I do not know where the corporeal eſſence in man ceaſes, or where the mental begins; though I admit of the diſtinction, becauſe the extreme differences can be clearly perceived. If we divide man into three parts, we ſhall be far from removing the difficulties, as we ſhould be were we even to follow thoſe modern philoſophers, who regard the thinking ſubject alone as the real Being (*Ego,*) and conſider all external appearances as confined to the ideas of conſcious beings. The greateſt and moſt peculiar difficulties in the philoſophic knowledge of the human ſubject conſiſts in this, that we have never yet been able clearly and diſtinctly to aſcertain the internal aſſociation of thoſe ſtriking differences which we obſerve in our being. Neither the moſt ſubtil phyſiology nor the fineſt ſpeculative philoſophy, have yet been able to explain the union of thought and phyſical operations. We may indeed doubt whether the labours of our German philoſophers, though founded jointly upon modern ſpeculation and modern chemiſtry, will be attended with any greater ſucceſs. Extreme caution is moſt undoubtedly requiſite to prevent our becoming too intimately and habitually acquainted with certain hypothetical notions reſpecting things really unknown, ſo as to miſtake them for truths and deduce erroneous concluſions.

and quite inaccurate.

Speculations on the nature of thought are deluſive.

It is much to be feared that the hypotheſes and poſtulates of ſpeculation will be of little value in this caſe; though to us they may ſeem very conſiſtent and clear, while we regard them only in a certain point of view. An attention to experimental proof may bring us nearer to our aim, though its perfect accompliſhment will perhaps never be within the reach of human inveſtigation. Experiments or facts may ſhew the corporeal as well as the mental functions in ſeveral lights, and in ſuch as we never can perceive by mere ſpeculation.

Though it is truly ſaid that the firſt principles of nature are placed beyond our reach, yet an endeavour to penetrate into the

Advantages of reſearch.

the interior of nature will always prove beneficial to the human mind; as long as we do not presume to have completely investigated the subject; but continue our exertions by uniting the observations of facts with deliberate reasoning.

The hypothesis of body and mind has led to many disquisitions respecting ghosts or spirits.

Since men have forgotten that what philosophy has separated is not on that account separated in nature, and since from the earliest ages, the mind and body of man have been considered as if distinct from each other, numberless questions have arisen which have given room for much controversy, without having met with any satisfactory answer. For example: Whether after the dissolution of the body, the spirit (or mind) continues to exist without the body? Whether the spirit can act without the body, and in what manner? And lastly, it is also a question, Whether, as we consider a disembodied spirit not only in a state of separate continual existence, but also in a state of continual existence and continual action *amongst us*, a mere spirit and its actions cannot become perceptible to our senses?—Whether the figure of a spirit (and in particular that of a deceased person) may not be seen? and, Whether a sound proceeding from it may not affect the ear of the living? All the knowledge usually considered as possible to be had of a departed spirit is confined to seeing and hearing; for as far as my information extends, the devil is the only spirit that enjoys the privilege of affecting the sense of smell at his departure.

Why the narratives of apparitions are generally concluded to be imposture.

We have less motive for disputing about the absolute possibility of seeing a spirit, because the idea of a spirit is so indistinct and vague, and because the words spirit and body in considering man, do in reality indicate mere relative notions. It is inconsistent with every known law of nature to suppose that those terms of relation adopted by us solely for the purpose of investigating the nature of man do themselves possess any separate and independent existence. This argument causes a suspicion of deceit or imposition always to attach to narratives of the apparitions of disembodied spirits. But those who are inclined to see and hear spirits, are not satisfied with this summary solution; they appeal to experience, against which no maxim *a priori* can hold. This only is required, that the experience must be true and well attested.

But the delusions of imagination or disease well deserve to be investigated.

Individuals who pretend to have seen and heard spirits are not to be persuaded that their apparitions were simply the creatures of their senses. You may tell them of the impositions that

that are frequently practiſed, and the fallacy which may lead us to take a ſpirit of our imagination by moon-light for a corpſe. We are generally adviſed to ſeize the ghoſts, in which caſe it is often found that they are of a very corporeal nature.—An appeal is alſo made to ſelf-deception, becauſe many perſons believe they actually ſee and hear where nothing is either to be ſeen or heard. No reaſonable man, I think, will ever deny the poſſibility of our being ſometimes deceived in this manner by our fancy, if he is in any degree acquainted with the nature of its operations. Nevertheleſs, the lovers of the marvellous will give no credit to theſe objections, whenever they are diſpoſed to conſider the phantoms of imagination as realities. We cannot therefore ſufficiently collect and authenticate ſuch proofs as ſhew how eaſily we are miſled; and with what deluſive facility the imagination can exhibit, not only to deranged perſons, but alſo to thoſe who are in the perfect uſe of their ſenſes, ſuch forms as are ſcarcely to be diſtinguiſhed from real objects.

Striking inſtances of apparitions ſeen by the author.

I myſelf have experienced an inſtance of this, which not only in a pſychological, but alſo in a medical point of view appears to me of the utmoſt importance. I ſaw, in the full uſe of my ſenſes, and (after I had got the better of the fright which at firſt ſeized me, and the diſagreeable ſenſation which it cauſed) even in the greateſt compoſure of mind, for almoſt two months conſtantly, and involuntarily, a number of human and other apparitions;—nay, I even heard their voices;—yet after all, this was nothing but the conſequence of nervous debility, or irritation, or ſome unuſual ſtate of the animal ſyſtem.

The publication of the caſe in the Journal of Practical Medicine, by Profeſſor Huſeland of Jena, is the cauſe of my now communicating it to the Academy. When I had the pleaſure of ſpending a few happy days with that gentleman laſt ſummer, at Pyrmont, I related to him this curious incident.

Narrative and remarks on ſpectres and apparitions.

But as it is probable he might not diſtinctly remember that which I had told altogether accidentally, perhaps indeed not very circumſtantially, ſome conſiderable errors have been admitted into his narrative. In ſuch a caſe, however, it is more neceſſary than in any other, to obſerve every thing with accuracy, and to relate it with fidelity and diſtinctneſs. I ſhall therefore paſs over nothing which I remember with any degree of certainty. Several incidents connected with the apparitions

paritions ſeem to me of great importance; though we might be apt to regard them in a ſecondary point of view; for we cannot determine of what conſequence even a circumſtance of the moſt trivial nature may be, if at any future period (in caſe more experiments of a like nature are aſcertained) ſome ſuppoſitions or concluſions can be made reſpecting the origin of ſuch phantoms, or on ſome law of the aſſociation of ideas according to which they are modified or follow one another.

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

I was alſo, which is ſeldom the caſe, in a ſituation to make obſervations on myſelf. I took down therefore in a few words what was moſt important, and recounted it immediately to ſeveral perſons. My memory, which is extremely retentive, has beſides treaſured up the moſt minute circumſtances; the more on that account, as this ſtory has very often proved the ſubject of my impartial conſideration, not only with regard to my own particular ſituation, but alſo in reſpect to its many pſychological conſequences. Its truth will, I hope, require no further aſſurance on my part, ſince a member of this academy (Mr. Selle) is an unexceptionable witneſs of it, having, as my phyſician, received a daily account of all that happened to me.

It would be extremely improper in an aſſembly like the preſent to ſpeak much of myſelf; it can only be excuſable in this particular caſe, where it ſerves to throw greater light on ſcientific inveſtigation. I muſt requeſt permiſſion therefore to notice ſeveral particulars of my ſituation previous to my ſeeing the phantoms, as thoſe incidents may have greatly affected the ſtate of my body and mind during that time.

In the laſt ten months of the year 1790, I underwent ſeveral very ſevere trials, which greatly agitated me. From the month of September in particular, repeated ſhocks of misfortune had befallen me, which produced the deepeſt ſorrow. It had been uſual for me to loſe blood by veneſection twice a year. This was done once on the 9th of July 1790, but towards the cloſe of the year it was omitted. In 1783 I had been ſuddenly ſeized with a violent giddineſs, which the phyſician imputed to an obſtruction in the ſmall muſcles of the *abdomen*, proceeding from too intenſe an application to ſtudy, and my ſedentary manner of life for many years. Theſe complaints were removed by a three years cure, and the rigid obſervance of a ſtrict diet during that time. In the firſt ſtage of the malady the application of leeches to the *anus* had been particularly effective,

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

fective, and this remedy I had from that time regularly applied twice or thrice a year, whenever I felt congeſtion in the head. It was on the 1ſt of March 1790 that the leeches had been laſt applied; the bleeding therefore and the clearing of the minuter blood-veſſels by leeches had, in 1790 been leſs frequently obſerved than uſual. A circumſtance too that could not tend to benefit my deplorable ſituation was, that from September I had been continually engaged in buſineſs which required the ſevereſt exertion, and which, from frequent interruptions, was rendered ſtill more burthenſome and diſtreſſing.

In the firſt two months of the year 1791, I was much affected in my mind by ſeveral incidents of a very diſagreeable nature; and on the 24th of February a circumſtance occurred which irritated me extremely. At ten o'clock in the forenoon my wife and another perſon came to conſole me; I was in a violent perturbation of mind, owing to a ſeries of incidents which had altogether wounded my moral feelings, and from which I ſaw no poſſibility of relief; when ſuddenly I obſerved at the diſtance of ten paces from me a figure,—the figure of a deceaſed perſon. I pointed at it, and aſked my wife whether ſhe did not ſee it. She ſaw nothing, but being much alarmed, endeavoured to compoſe me, and ſent for the phyſician. The figure remained ſome ſeven or eight minutes, and at length I became a little more calm; and as I was extremely exhauſted, I ſoon afterwards fell into a troubled kind of ſlumber, which laſted for half an hour. The viſion was aſcribed to the great agitation of mind in which I had been, and it was ſuppoſed I ſhould have nothing more to apprehend from that cauſe; but the violent affection had put my nerves into ſome unnatural ſtate, from this aroſe further conſequences, which require a more detailed deſcription.

In the afternoon, a little after four o'clock, the figure which I had ſeen in the morning again appeared. I was alone when this happened; a circumſtance which, as may be eaſily conceived, could not be very agreeable. I went therefore to the apartment of my wife, to whom I related it. But thither alſo the figure purſued me. Sometimes it was preſent, ſometimes it vaniſhed, but it was always the ſame ſtanding figure. A little after ſix o'clock ſeveral ſtalking figures alſo appeared; but they had no connection with the ſtanding figure. I can aſſign no other reaſon for this apparition than that, though much more

compoſed

compoſed in my mind, I had not been able ſo ſoon entirely to forget the cauſe of ſuch deep and diſtreſſing vexation, and had reflected on the conſequences of it, in order, if poſſible, to avoid them; and that this happened three hours after dinner, at the time when the digeſtion juſt begins.

Narrative and remarks on ſpectres produced by nervous imagination.

At length I became more compoſed with reſpect to the diſagreeable incident which had given riſe to the firſt apparition; but though I had uſed very excellent medicines, and found myſelf in other reſpects perfectly well, yet the apparitions did not diminiſh, but on the contrary rather encreaſed in number, and were transformed in the moſt extraordinary manner.

After I had recovered from the firſt impreſſion of terror, I never felt myſelf particularly agitated by theſe apparitions, as I conſidered them to be what they really were, the extraordinary conſequences of indiſpoſition; on the contrary, I endeavoured as much as poſſible to preſerve my compoſure of mind, that I might remain diſtinctly conſcious of what paſſed within me. I obſerved theſe phantoms with great accuracy, and very often reflected on my previous thoughts, with a view to diſcover ſome law in the aſſociation of ideas, by which exactly theſe or other figures might preſent themſelves to the imagination.—Sometimes I thought I had made a diſcovery, eſpecially in the latter period of my viſions; but on the whole I could trace no connexion which the various figures that thus appeared and diſappeared to my ſight had, either with my ſtate of mind, or with my employment, and the other thoughts which engaged my attention. After frequent accurate obſervations on the ſubject, having fairly proved and maturely conſidered it, I could form no other concluſion on the cauſe and conſequence of ſuch apparitions than that, when the nervous ſyſtem is weak and at the ſame time too much excited, or rather deranged, ſimilar figures may appear in ſuch a manner as if they were actually ſeen and heard; for theſe viſions in my caſe were not the conſequence of any known law of reaſon, of the imagination, or of the otherwiſe uſual aſſociation of ideas; and ſuch alſo is the caſe with other men, as far as we can reaſon from the few examples we know.

The origin of the individual pictures which preſent themſelves to us, muſt undoubtedly be ſought for in the ſtructure of that organization by which we think; but this will always.

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

always remain no leſs inexplicable to us than the origin of thoſe powers by which conſciouſneſs and fancy are made to exiſt.

The figure of the deceaſed perſon never appeared to me after the firſt dreadful day; but ſeveral other figures ſhewed themſelves afterwards very diſtinctly; ſometimes ſuch as I knew, moſtly, however, of perſons I did not know, and amongſt thoſe known to me, were the ſemblances of both living and deceaſed perſons, but moſtly the former: and I made the obſervation that acquaintance with whom I daily converſed never appeared to me as phantaſms; it was always ſuch as were at a diſtance. When theſe apparitions had continued ſome weeks, and I could regard them with the greateſt compoſure, I afterwards endeavoured, at my own pleaſure to call forth phantoms of ſeveral acquaintance, whom I for that reaſon repreſented to my imagination in the moſt lively manner, but in vain.—For however accurately I pictured to my mind the figures of ſuch perſons, I never once could ſucceed in my deſire of ſeeing them *externally;* though I had ſome ſhort time before ſeen them as phantoms, and they had perhaps afterwards unexpectedly preſented themſelves to me in the ſame manner. The phantaſms appeared to me in every caſe involuntarily, as if they had been preſented externally, like the phenomena in nature, though they certainly had their origin internally; and at the ſame time I was always able to diſtinguiſh with the greateſt preciſion phantaſms from phenomena. Indeed, I never once erred in this, as I was in general perfectly calm and ſelf-collected on the occaſion. I knew extremely well, when it only appeared to me that the door was opened, and a phantom entered, and when the door really was opened and any perſon came in.

It is alſo to be noted, that theſe figures appeared to me at all times, and under the moſt different circumſtances, equally diſtinct and clear. Whether I was alone, or in company, by broad day-light equally as in the night time, in my own as well as in my neighbour's houſe; yet when I was at another perſon's houſe, they were leſs frequent, and when I walked the public ſtreet they very ſeldom appeared. When I ſhut my eyes ſometimes the figures diſappeared, ſometimes they remained even after I had cloſed them. If they vaniſhed in the former caſe, on opening my eyes again, nearly the ſame figures appeared which I had ſeen before.

I ſome-

I ſometimes converſed with my phyſician and my wife, concerning the phantaſms which at the time hovered around me; for in general the forms appeared oftener in motion than at reſt. They did not always continue preſent—they frequently left me altogether, and again appeared for a ſhort or longer ſpace of time, ſingly or more at once; but, in general, ſeveral appeared together. For the moſt part I ſaw human figures of both ſexes; they commonly paſſed to and fro as if they had no connection with each other, like people at a fair where all is buſtle; ſometimes they appeared to have buſineſs with one another. Once or twice I ſaw amongſt them perſons on horſeback, and dogs and birds; theſe figures all appeared to me in their natural ſize, as diſtinctly as if they had exiſted in real life, with the ſeveral tints on the uncovered parts of the body, and with all the different kinds and colours of clothes. But I think, however, that the colours were ſomewhat *paler* than they are in nature.

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

None of the figures had any diſtinguiſhing characteriſtick, they were neither terrible, ludicrous, nor repulſive; moſt of them were ordinary in their appearance,—ſome were even agreeable.

On the whole, the longer I continued in this ſtate, the more did the number of phantaſms encreaſe, and the apparitions became more frequent. About four weeks afterwards I began to hear them ſpeak: ſometimes the phantaſms ſpoke with one another; but for the moſt part they addreſſed themſelves to me: theſe ſpeeches were in general ſhort, and never contained any thing diſagreeable. Intelligent and reſpected friends often appeared to me, who endeavoured to conſole me in my grief, which ſtill left deep traces on my mind. This ſpeaking I heard moſt frequently when I was alone: though I ſometimes heard it in company, intermixed with the converſation of real perſons; frequently in ſingle phraſes only, but ſometimes even in connected diſcourſe.

Though at this time I enjoyed rather a good ſtate of health both in body and mind, and had become ſo very familiar with theſe phantaſms, that at laſt they did not excite the leaſt diſagreeable emotion, but on the contrary afforded me frequent ſubjects for amuſement and mirth; yet as the diſorder ſenſibly encreaſed, and the figures appeared to me for whole days together, and even during the night, if I happened to awake,

I had

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

I had recourſe to ſeveral medicines, and was at laſt again obliged to have recourſe to the application of leeches to the anus.

This was performed on the 20th of April at eleven o'clock in the forenoon. I was alone with the ſurgeon, but during the operation, the room ſwarmed with human forms of every deſcription, which crouded faſt one on another; this continued till half paſt four o'clock, exactly the time when the digeſtion commences. I then obſerved that the figures began to move more ſlowly; ſoon afterwards the colours became gradually paler; every ſeven minutes they loſt more and more of their intenſity, without any alteration in the diſtinct figure of the apparitions. At about half paſt ſix o'clock all the figures were entirely white, and moved very little; yet the forms appeared perfectly diſtinct; by degrees they became viſibly leſs plain, without decreaſing in number, as had often formerly been the caſe. The figures did not move off, neither did they vaniſh which alſo had uſually happened on other occaſions. In this inſtance they diſſolved immediately into air; of ſome even whole pieces remained for a length of time, which alſo by degrees were loſt to the eye. At about eight o'clock there did not remain a veſtige of any of them, and I have never ſince experienced any appearance of the ſame kind. Twice or thrice ſince that time I have felt a propenſity, if I may be ſo allowed to expreſs myſelf, or a ſenſation as if I ſaw ſomething which in a moment again was gone. I was even ſurpriſed by this ſenſation whilſt writing the preſent account, having, in order to render it more accurate, peruſed the papers of 1791, and recalled to my memory all the circumſtances of that time. So little are we ſometimes, even in the greateſt compoſure of mind, maſters of our imagination.

This is an exact narrative of the apparitions which I obſerved during the diſordered ſtate of my nerves: and I ſhall now add a few obſervations, partly with the intention of explaining their origin from other obſervations made on myſelf, and partly with a view of pointing out at leaſt ſome diſtant pſychological conſequences, which might be deduced from this remarkable caſe.

Experience ſhews that we may, in various manners, imagine that we ſee figures, and even hear them when they do not really exiſt.

1ſt

1ſt. And commonly this may happen in dreams.—The manner of dreaming is different in every individual, and probably depends on the joint effects of the powers of intellect, and thoſe by which the impreſſions of the ſenſes are received, and theſe are modified by the ſtate of the ſyſtem at each particular time. I have myſelf made ſome remarkable obſervations on the nature of my dreams, and compared them with ſome obſervations on that ſubject which have been communicated to me by others. Narrative and remarks on ſpectres produced by nervous indiſpoſition.

2d. In every degree of mental derangement till abſolute inſanity.

3d. In fevers of the brain, which for a ſhort time, or at certain intermitting periods, occaſion a delirium.

4th. By the mere power of imagination without any fever, when in other reſpects the judgment is perfectly ſound. In this caſe it is very difficult to diſcover the truth, unleſs we combine an accurate habit of obſervation with the moſt impartial ſcrutiny.

Inſtances are too frequent in which we are impoſed upon, not by the imagination, but by deluſion of the judgment. How many are there, who prefer the marvellous and aſſume an air of importance, when they have an opportunity of relating wonderful things of themſelves—How few are there who endeavour to diveſt themſelves of prejudice, or to check their imagination; and ſtill fewer are they who are accurate in their obſervations, eſpecially in ſuch as relate to themſelves; even thoſe who have ſufficient firmneſs to adhere ſtrictly to the truth form an inconſiderable number. Hence it is, that when a perſon relates any ſtrange incident, he either detracts or magnifies, and will even fancy that he has verified ſome facts, which he has invented only at the moment that he relates them. This laſt is the caſe with a claſs of men who obſtinately perſiſt in their own opinions, and frequently aſſert more than they can ſupport, merely with a view to maintain what they have once advanced. All the above mentioned circumſtances ſeem to have coincided in the celebrated viſions of Emanuel Swedenborg. He delighted in ſpeculation and myſtical theology; he had formed a ſyſtem for himſelf in which ghoſts were neceſſary, and it was his primary view to eſtabliſh this wonderful ſyſtem. It is poſſible that he may have ſeen phantaſms, the more ſo as he

ſtudied

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ſtudied much, and was a great eater.* But in order to appear a prodigy to the world, he embelliſhed his viſions on which he wrote voluminous treatiſes, by creating new images in conformity with his own ſyſtem.

Laſtly, thoſe who are moſt converſant with the marvellous, give but a very indiſtinct idea of their viſions. This I have found in converſation with perſons who in other reſpects were very worthy characters, but who were great admirers of the what are termed occult ſciences, which they cultivated to ſuch a degree, that to give you a notion of it here would ſeem prepoſterous. I have frequently diſcourſed on ſpirits, and the ſeeing of ſpirits, with a perſon who ranked very high in the ſchool of ſecret wiſdom, but who was otherwiſe a man of a very limited capacity, and rather ignorant in all thoſe ſciences which enlighten the mind. This perſon told me amongſt other things that he ſhould feel very unhappy, were he not continually in company with ſpirits. As I have always taken a pleaſure in the clear developement of human opinions, however abſurd they may appear, I was deſirous to learn in what manner he ſaw the ſpirits, and how he came into company with them?—But here he would not allow of the appearance of any corporeal forms; he aſſured me that ſpirits were only to be ſeen with the eyes of the ſpirit: then he added in a very ſerious tone, "Juſt as the human ſoul is *Naephaeſch*, "or a branch taken off the tree, ſo are all ſpirits branched off "from the ſupreme ſpirit, as it in the aſtringent motion com-"preſſed its being." On nearer enquiry I could eaſily perceive that he entertained a confuſed notion of the cabaliſtick ontology of Spinoza, and that he imagined all the powers in nature to be ſpirits. What he meant to ſay therefore, was neither more nor leſs than that he ſhould feel unhappy, did he live in a world where nature was perfectly inanimate; if he could not think that every thing around him was in the continual and mutual *exerciſe* of its powers. In this belief then he peopled all ſpace with ſpirits, nearly in the ſame manner as the antient mythology peopled the woods with Dryads and

* On this ſubject, the review of Swedenborg's Works in the *Algemeine Deutſche Bibliothek*, vol. 107, p. 15, is very intereſting. In it the reſemblance of Swedenborg's ſyſtem, with the viſions of the German enthuſiaſt, Johann Tennhart, is clearly accounted for: he alſo was a great eater.

Hamadryads. Indeed, every thing properly conſidered, the opinion of my cabaliſt is not quite ſo very abſurd as you may ſuppoſe; for in reality, the word *power* is with the philoſopher only that which the × is to the mathematician; and, if I be not altogether miſtaken, the mathematician can with his ×, bring more clear truths to light, than the philoſopher by the word *power*. If a given power cannot be rendered ſubſervient to deduction, ſo that, like Newton's *calculus*, it ſhall perfectly accord with experience; nothing more will be determined or explained by the mere word power, than by the word ſpirit; and I doubt much whether the new judicious Kantian ſyſtem of Dynamic natural philoſophy, which conſiders all bodies as mere aggregates of powers, would not rather cut the gordian knot than unravel it.

Narrative and remarks sn ſpectres produced by nervous indiſpoſition.

It is not very uncommon that by a derangement of the corporeal powers, even without inſanity and inflammatory fevers. apparitions do ſtrike the eye externally, which are only internally the production of the imagination. The experience of this may teach us a leſſon of forbearance, not raſhly to conſider as impoſtors thoſe well diſpoſed perſons who believe they have ſeen apparitions. But as manifold experience ſhews us how far the human imagination can go in the external repreſentation of pictures; it may alſo admoniſh thoſe well-diſpoſed perſons not to aſcribe to their viſions any degree of reality, and ſtill leſs to conſider the effects of a diſordered ſyſtem, as proofs that they are haunted by ſpirits.

The celebrated Juſtus Moſer frequently believed that he ſaw flowers. Another of my acquaintance ſees in like manner, at times, mathematical figures, circles, ſquares, &c. in different colours. More examples of this kind may perhaps be found in Moretz's Magazine, in Krueger's Experimental Pſychology, and in Bonnet's Pſychological writings. The hearing of ſounds is a caſe which ſeldomer occurs. My much-lamented friend Moſes Meudeljohn had, in the year 1792, by too intenſe an application to ſtudy, contracted a malady, which alſo abounded with particular pſychological apparitions. For upwards of two years he was incapacitated from doing any thing; he could neither read nor think, and was rendered utterly incapable of ſupporting any loud noiſe. If any one talked to him rather in a lively manner, or if he himſelf happened to be diſpoſed to lively converſation, he

fell

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

fell in the evening into a very alarming ſpecies of catalepſis, in which he ſaw and heard every thing that paſſed around him, without being able to move a limb. If he had heard any lively converſation during the day, a ſtentorian voice repeated to him while in the fit, the particular words or ſyllables that had been pronounced with an impreſſive accent, or loud emphatic tone, and in ſuch a manner that his ears reverberate.

Seldom as it may happen, that perſons believe they ſee human forms, yet examples of the caſe are not wanting. A reſpectable member of this academy, diſtinguiſhed by his merit in the ſcience of botany, whoſe truth and credibility are unexceptionable, once ſaw in this very room in which we are now aſſembled, the phantaſm of the late preſident Maupertuis. A perſon of a ſound and unprejudiced mind, though not a man of letters, whom I know well, and whoſe word may be credited, related to me the following caſe. As he was recovering from a violent nervous fever, being ſtill very weak, he lay one night in bed perfectly conſcious that he was awake, when the door ſeemed to open, and the figure of a woman entered, who advanced to his bed-ſide. He looked at it for ſome moments, but as the ſight was diſagreeable, he turned himſelf and awakened his wife; on turning again however he found the figure was gone. But out of many caſes I have never known an inſtance like my own, in which any perſon had for almoſt two months conſtantly beheld ſuch viſionary forms, and ſeemed even to have heard them; except it was that of two young ladies, who, as I have been credibly informed, frequently ſaw appearances of this nature.

I am by no means inſenſible to a certain feeling which admoniſhes me of the impropriety of talking ſo much of myſelf in an aſſembly like this; but ſince I tranſgreſs only with a ſcientific intention, to contribute to the knowledge of the effects of the human imagination, I muſt endeavour to ſuppreſs this feeling. I may look for pardon, I truſt, from thoſe who know and reſpect every thing which tends to enlarge the ſtock of human knowledge, even if I ſpeak more of myſelf. For, when I proceed to deſcribe the ſtate of my imagination, and the nature of the apparitions during a previous malady, it will be merely with an intention to ſhew the apparitions which form the ſubject of this lecture in a leſs wonderful point of view,

and

and by that means perhaps to contribute in ſome degree to the illuſtration of ſo ſtrange an incident.

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

I muſt obſerve that my imagination poſſeſſes in general a great facility in picturing. I have for example ſketched in my mind a number of plans for novels and plays; though I have committed very few of them to paper, becauſe I was leſs ſolicitous to execute than to invent. I have generally arranged theſe outlines when, in a chearful ſtate of mind. I have taken a ſolitary walk, or when travelling I have ſat in my carriage, and could only find employment in myſelf and my imagination. Conſtantly and even now do the different perſons whom I imagine in the formation of ſuch a plot, preſent themſelves to me in the moſt lively and diſtinct manner: their figure, their features, their manner, their dreſs, and their complexion, are all viſible to my fancy. As long as I meditate on a fixed plan, and afterwards carry it into effect,—even when I am often interrupted, and muſt begin it again at different times, all the acting perſons continue preſent in the very ſame form in which my imagination at firſt produced them. I find myſelf frequently in a ſtate betwixt ſleeping and waking, in which a number of pictures of every deſcription, often the ſtrangeſt forms, ſhew themſelves, change and vaniſh. In the year 1778, I was afflicted with a bilious fever, which, at times, though ſeldom, became ſo high as to produce delirium. Every day towards evening, the fever came on, and if I happened to ſhut my eyes at that time, I could perceive that the cold fit of the fever was beginning even before the ſenſation of cold was obſervable. This I knew by the diſtinct appearance of coloured pictures of leſs than half their natural ſize, which looked as if in frames. They were a ſet of landſcapes compoſed of trees, rocks, and other objects. If I kept my eyes ſhut, every minute ſome alteration took place in the repreſentation. Some figures vaniſhed, and others appeared. But if I opened my eyes all was gone; if I ſhut them again I had quite a different landſcape. This caſe was therefore entirely different from what afterwards in the year 1791, when the figure remained unchanged during the opening and ſhutting of the eyes. In the cold fit of the fever I ſometimes opened and ſhut my eyes every ſecond for the purpoſe of obſervation, and every time a different picture appeared replete with various objects

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jects which had not the leaſt reſemblance with thoſe that appeared before. Theſe pictures preſented themſelves without interruption, as long as the cold fit of the fever laſted. They became fainter as ſoon as I began to grow warm, and when I was perfectly ſo, all were gone. When the cold fit of the fever was entirely paſt, no more pictures appeared; but if on the next day I could again ſee pictures when my eyes were ſhut, it was a certain ſign that the cold fit was coming on. I muſt further obſerve, that when I either think deeply on a ſubject, or write attentively, particularly when I have exerted myſelf for ſome time, a thought frequently offers itſelf which has no connection with the work before me, and this at times in a manner ſo very lively, that it ſeems as if expreſſed in actual words.

This natural vivacity of imagination renders it leſs wonderful, that after a violent commotion of mind, a number of deluſive pictures ſhould appear for ſeveral weeks in ſucceſſion. Their leaving me on the application of leeches, ſhews clearly that ſome anomaly in the circulation of the blood was connected with the appearance of thoſe phantaſms; though it may perhaps be too haſty a concluſion to ſeek for their cauſe in that alone. It ſeems likewiſe remarkable, that the beginning of the apparitions, after the diſturbance in my mind was ſettled, as well as the alteration which took place when they finally left me, happened exactly at the time when digeſtion commenced. It is no leſs remarkable, that the apparitions before they entirely ceaſed, loſt their intenſity of colours; and that they did not vaniſh or change as formerly, but ſeemed gradually to diſſolve into air.

Had I not been able to diſtinguiſh phantaſms from phenomena, I muſt have been inſane. Had I been fanatic or ſuperſtitious, I ſhould have been terrified at my own phantaſms, and probably might have been ſeized with ſome alarming diſorder. Had I been attached to the marvellous, I ſhould have ſought to magnify my own importance, by aſſerting that I had ſeen ſpirits; and who could have diſputed the facts with me? The year 1791 would perhaps have been the time to have given importance to theſe apparitions. In this caſe however, the advantage of ſound philoſophy, and deliberate obſervation may be ſeen. Both prevented me from becoming either a lunatic or an enthuſiaſt; with nerves ſo ſtrongly excited

cited, and blood ſo quick in circulation, either misfortune might have eaſily befallen me. But I conſidered the phantaſms that hovered around me as what they really were, namely, the effects of diſeaſe; and made them ſubſervient to my obſervations, becauſe I conſider obſervation and reflection as the baſis of all rational philoſophy.

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Our modern German philoſophers, will not allow that obſervation ought to be admitted in theoretical philoſophy. Hence aroſe Kants' Tranſcendental Idealiſm, which at laſt degenerated into the groſs enthuſiaſtic idealiſm; which is found in Fichte's writings. This philoſopher conſiders all external objects as our own productions. "What we con-"ſider as things independent of us are," according to him, "no more than our own creatures, which we fear, admire "and deſire; we believe our fate to be dependent on a "ſhadow, which the ſingle breath of a free being might "deſtroy." Theſe are Mr. Fichte's own words *.

The mere picture in the mind, without external experience, would never be ſufficient to afford us a convincing proof, whether we ſaw phenomena or phantaſms. The critical philoſophers maintain, that knowledge deduced from obſervation is merely empirick, and therefore not to be depended on; it is perhaps true that nature has aſſigned us no greater certainty than this reſpecting our ideas. But could we be truly conſcious of our grounds of reaſon, if the appearances called external, which follow laws that do not depend on the repreſentations in our mind, did not continually agree with thoſe repreſentations? Are we poſſeſſed of any other criterion? Does not the great theoretical philoſopher, when he ſees every thing yellow, conclude that his eye is jaundiced; or when every thing appears black to him, that his brain is affected? In theſe caſes he does not truſt his imagination or mental powers alone.

I may here apply the conſideration of the illuſions which I witneſſed. I am well aware that no general concluſions can be drawn from a ſingle inſtance; but ſtill the experience of a ſingle caſe, if accurately obſerved and faithfully deſcribed, is ſufficient to deſtroy hypotheſes which have too long been honoured with the name of ſyſtems.

* Fichte's Appeal, p. 44.

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According to Fichte, ſince during the ſituation I have above deſcribed, I was in other reſpects in the perfect uſe of my reaſon, as well as the perſons who were really about me; as the apparitions which I ſaw, as well as thoſe which are conſidered as realities, were the one as well as the other, my own productions:—Why then were my creatures of both kinds ſo eſſentially different?

My judgment ſhewed me this plainly, by concluſions founded on the previous courſe of obſervations. The greateſt modern idealiſts who depend ſo much on the confuſion in which they have involved themſelves by the ſuppoſed depth of their ſpeculations, will certainly never pretend that both perceptions were of the ſame nature; ſince if ſo, I could not have inveſtigated their difference? But by what means could this be done? I obſerved that real perſons followed in a determinate order, by external laws that do not depend on me, in an order that I myſelf muſt continually follow, as was evident from my ſenſe of conſciouſneſs. I could alſo lay hold of the real objects, as well as of myſelf. Neither of theſe circumſtances was, however, the caſe with the phantaſms; I had always found it ſo in the conſtant obſervation of myſelf, of the apparitions without me, and in my own conſciouſneſs.

The phantaſms, as well as the phenomena, no doubt, lay in my mind; but I am neceſſarily compelled to aſcribe to the latter, the ſame reality which I am obliged to aſcribe to myſelf: viz. ſomething that does not lie in my mind alone; ſomething that alſo exiſts without my mind; ſomething independent of my conſciouſneſs, which determines the nature of my idea; ſomething which we formerly uſed to call *the thing itſelf*, before the critical philoſophy ſo unjuſtly reprobated this unexceptionable term. On the contrary, however, I could not aſcribe this ſame reality to the illuſion; I could form no other concluſion, than that they originated in my internal conſciouſneſs alone; in a conſciouſneſs which was alſo diſordered, as I might juſtly conclude from the obſervations I made on myſelf. I repeat, that both the phenomena and the phantaſms exiſted in my mind: if I had not been able to diſtinguiſh between them, I muſt have been inſane. By what means could I diſtinguiſh, if I did not attribute reality to the former;—and that they poſſeſſed reality, I inferred from obſervations

ſervations to which I am ſtill inclined to give confidence, until Mr. Fichte can more clearly convince me that it ought in no caſe to be depended on.

III.

Analyſis of Ambergris; by Cit. BOUILLON LA GRANGE *.

IT is an opinion now generally adopted, that ambergris is formed in the ſtomach of the cachalot, or ſpermaceti whale, *phyſeter macrocephalus*, and appears to be a product of its digeſtive faculties.

Ambergris found in the ſtomach of the phyſeter macrocephalus.

Dr. Swediaur has ſhewn, in his inquiries into the nature and origin of ambergris, that the beaks of the cuttlefiſh, interſperſed throughout all the large pieces of ambergris, that are found ſwimming on the ſea, or caſt upon the ſhore, as well as thoſe extracted from the bellies of whales, belong to the ſpecies called by Linneus *ſepia octopodia*. The exiſtence of theſe beaks and other foreign ſubſtances in ambergris evidently proves it to have been originally in a ſoft or fluid ſtate. Dr. Swediaur aſſerts, that the whale, in the belly of which ambergris is found, is the ſame ſpecies as that from which ſpermaceti is extracted, which appears to be the *phyſeter macrocephalus* of Linneus; and feeds chiefly on the large ſpecies of cuttlefiſh. The ambergris is found in the inteſtinal canal of this fiſh; it is a ſource of diſeaſe to it †; and after it iſſues from the cavity in which it had been included, it gradually acquires the ſolidity it is known to poſſeſs.

Beaks of cuttle-fiſh found in all the large pieces;

whence it muſt once have been ſoft, if not fluid.

The ſpermaceti whale that produces it.

Occaſions a diſeaſe in the fiſh? becomes ſolid after its excluſion.

Ambergris is found in the Indian Seas, near the Moluccas, Maldivia Iſlands, and Madagaſcar, on the coaſts of China and Japan, and from Jolo to the Philippine Iſlands. It is frequently collected on the ſhores of the Iſland of Maragnan, or of Brazil; but more commonly on thoſe of Africa, toward cape Blanco, the gulf of Arguin, the bay of Portendie, and on ſome other Iſlands, that extend from Moſambique to the Red Sea.

Found on the ſhores of the Indian Ocean and its iſlands.

Of Brazil, and alſo of Africa.

* *Annales de Chimie*, No. 139. or XLVII. 68.

† Is it not rather the effect, than the cauſe of diſeaſe? J. C.

The inhabitants of the Samballas ſeek for it on the ſhore after ſtorms by the ſmell.

From the accounts of various travellers, the inhabitants of the Samballas ſeek for it in a ſingular manner: they hunt it by ſcent. After a ſtorm they run along the ſhore, and if any ambergris be thrown up, they find it by the ſmell. There are certain birds and other animals on thoſe coaſts, that are very fond of ambergris, and, attracted from a diſtance by its ſmell, they ſearch for it to eat.

Certain birds and other animals fond of it.

Certainly a vegetable production.

Excrements of ſome animals, particularly of the ox and pig, reſemble it in ſmell.

Cowdung called in ſome places native muſk.

There is no doubt, that ambergris is a vegetable production. Many ſubſtances reſemble it greatly in ſmell, ſuch as the excrements of mammiferous animals, particularly thoſe of the ox and the pig. I have found, that cowdung dried in the ſun, has a ſmell much like that of ambergris, and even of muſk, whence in ſome countries this ſubſtance, ſo prepared, has received the name of *native muſk*.

External qualities of ambergris.

Its odour more powerful as it grows old, or when mixed with other perfumes.

Ambergris, *ambra griſea*, is a light ſubſtance, ſwimming on water, ſolid, opaque, of an aſhen gray colour ſtreaked with white and yellowiſh brown, ſlightly odoriferous, its odour diſplaying itſelf more as it grows old, or when it is mixed with muſk or other aromata, as is done in preparing perfumes or odoriferous waters.

Marks of good ambergris.

In its natural ſtate good ambergris is known by adhering like wax to the edge of a knife with which it is ſcraped, retaining the impreſſion of the teeth or nails, and emitting a fat odoriferous liquid on being penetrated with a hot needle. Though ſolid, and in general brittle, it is not hard enough to take a poliſh; but on rubbing it with the nail it becomes as ſmooth as hard ſoap.

The older chemiſts claſſed it among the bitumens.

Geoffroy's analyſis of it by alcohol,

Geoffroy, Neumann, Grim, and Brow, have claſſed ambergris among the bitumens. The analyſis made of it by theſe chemiſts was inadequate to determine its nature. Ambergris, ſays Geoffroy, melts into a reſin of a yellow or gold colour; kindles, and burns with flame. Spirit of wine does not diſſolve it entirely; a black ſubſtance like pitch being left, on which it does not act. When it is diſſolved, it lets fall after ſome time a white cloudy ſediment, which gradually coagulates, and grows thicker and thicker. This *coagulum*, on drying, changes to a ſhining foliated earth, nowiſe different from ſpermaceti.

and by diſtillation.

On diſtillation, according to the ſame chemiſt, ambergris yields at firſt an inſipid phlegm, then an acid ſpirit or liquor

and

and a very odoriferous yellow oil, with a ſmall portion of a volatile acidoſaline ſalt; and laſtly, a ſhining black bituminous ſubſtance remains at the bottom of the retort. Hence we ſee this analyſis, which does not differ from thoſe related by all other chemiſts, requires to be reviſed, in order to give us determinate ideas of the nature of this ſingular ſubſtance.

This analyſis inſufficient.

It is perhaps neceſſary to appriſe thoſe, who wiſh to repeat theſe experiments, that they ſhould pay great attention to the choice of the ambergris. Many varieties are found in the ſhops, the different kinds of which are diſtinguiſhed by their price. No doubt this ſubſtance is fabricated, as caſtor is in ſome parts of Germany. Bayen aſſured me, that he had ſeen it made at Frankfort; and it is well known that this father of chemiſtry ſaw clearly, and that his memory was not apt to deceive him; and, what is very rare among travellers, that he never told a lie.

Neceſſary to be very careful in choofing it. Many varieties of it in the ſhops.

Fabricated by art, as Bayen ſaw at Frankfort.

I have examined ſeveral ſpecimens of the ambergris of the ſhops: ſome varied in ſpecific gravity, were more or leſs deep in colour, had very little ſmell, and were flexible; others were of an aſhen gray colour, and tolerably hard; and ſome were almoſt ſtony, ſcarcely at all ſoluble in alcohol, and void of ſmell.

Differences of theſe varieties.

The ambergris I analyſed was not purchaſed from the ſhops; and, on comparing it with that in the cabinet of the Muſeum, I could find no difference, either in colour or in ſmell.

Phyſical Properties.

It is of an aſhen gray colour, internally variegated with a few yellow ſtreaks, of a ſweet and pleaſing ſmell, ſoftening between the fingers; when reduced to a fine powder it is of a deeper colour; pounded in a glaſs mortar it agglutinates, and adheres to the peſtle.

Its colour, ſmell, texture,

Of a flat and almoſt inſipid taſte, exhibiting the ſame appearances as wax when chewed between the teeth.

taſte,

Its ſpecific gravity is to that of water as 844 or 849 to 1000.

ſpecific gravity.

According to Briſſon, the ſpecific gravity of ambergris is 9263; the weight of the French cubic inch, 4 gros 58 grs.; that of the cubic foot, 64 lbs. 14 oz. 3 gr. 47 grs. *

* The ſpecimens of ambergris, on which Briſſon made his experiments, were taken from the king's collection.

The

The ſpecific gravity of the blackiſh gray ambergris 7803; the weight of the cubic inch, 4 gros 3 grs.; that of the cubic foot, 54 lbs. 9 oz. 7 gr. 35 grs.

Chemical Properties.

It burns entirely away.

Experiment I. Ambergris burns, and is entirely diſſipated, when placed on a red hot coal. It leaves behind an agreeable ſmell.

Melts with a leſs degree of heat,

If the combuſtion be conducted more ſlowly, in a crucible of platina, the ambergris melts, diffuſing the ſame ſmell. The ſmell of a fatty ſubſtance may be diſtinguiſhed likewiſe.

Nothing remains in the crucible, but a greaſy black ſpot.

and is then a ſhining brown fluid.

50° of Reaumur's thermometer are ſufficient to melt it, and a ſhining brown fluid is thus obtained.

Becomes volatile at 80° R.

At 80° it is volatilized in the form of a white vapour.

Its ſmell indicates an acid.

Exp. II. The ſmell perceived during its volatilization having led me to ſuſpect the preſence of an acid analogous to that of balſams, an experiment was made to aſcertain this.

This acid detected by evaporating it under a bell,

A bit of ambergris was placed in a china capſule, covered with a bell, in which was ſuſpended ſome litmus paper. This apparatus being placed on a ſand-heat, the temperature was raiſed ſufficiently to volatilize the ambergris, and the paper was very quickly reddened. Nothing now remained but to determine the nature of the acid; and for this purpoſe

and proved to be the benzoic.

Schelee's proceſs for extracting the acid of Benjamin was adopted.

The product was examined, and left no doubt of their analogy.

On diſtillation,

Exp. III. The analyſis by diſtillation in a retort added nothing to the knowledge we already poſſeſſed of the nature of ambergris.

it gives out a whitiſh acid liquor, with a light oil, and leaves a bulky coal.

A gentle heat melted it: on raiſing the fire it was decompoſed, and there paſſed over into the receiver a whitiſh acid liquor with a white oil, partly ſoluble in alcohol, which gave it a yellow colour. In the retort remained a light and very bulky coal.

Imparts neither taſte nor ſmell to cold water,

Exp. IV. Ambergris ſwims on water, and is not penetrated by it when cold. It imparts to it neither taſte nor ſmell.

Boiling

Boiling water is equally incapable of altering its properties. In this degree of heat the ambergris melts, and appears in the form of a browniſh oily fluid; and a ſmall quantity of black matter, inſoluble in alcohol, ſeparates from it. The filtered liquor has neither colour nor ſmell, it has however a ſlightly bitteriſh taſte. and to boiling water it gives only a ſlight bitter taſte.

It is only in conſequence of the temperature therefore that the ambergris melts, ſince on this being lowered it reſumes the ſame properties as before.

Exp. V. Acids in general have little action upon ambergris. Theſe agents likewiſe do not enable us to diſcover the conſtituent parts of this compound ſubſtance. Acids act but feebly on it.

Dilute ſulphuric acid effects no change in it. The concentrated acid expoſes a little oxide of carbon. Sulphuric.

The ſame phenomena are produced by the muriatic and oxigenated muriatic acid. Muriatic.

The nitric acid, at 18°, diſtilled over this ſubſtance in the pneumato-chemical apparatus, produces nitrous gas, carbonic acid, and azote gas. Nitric produces nitrous gas, carbonic acid, and azote gas;

The azote gas ariſes no doubt from the decompoſition of ſome animal matters, accidentally mixed with the ambergris, as may be obſerved in the examination of ſome pieces.

After the extraction of the elaſtic fluids, a thick liquor, inclining to a yellow colour, was found in the retort: this, on bringing it to a ſoft conſiſtency, ſlightly ſwelled up; and being evaporated to dryneſs, in a porcelain capſule, what remained was a dry, bitter ſubſtance, of a golden yellow hue, ſhining and tranſparent, and exhibiting properties analogous to thoſe of reſins. and leaves a ſubſtance analogous to reſins.

Exp. VI. Alcalis combine with ambergris, and form with it ſoluble ſoaps. Alcalis form ſoap with ambergris.

Into a crucible of platina were put one gramme, 592 (30 fr. grs.) of ambergris, with 531 thouſandth of a gramme (10grs.) of pure potaſh; it was gently heated; the mixture melted, without exhibiting any ſigns of the preſence of ammonia; on cooling a homogeneal browniſh maſs was obtained.

On this were poured 30 grammes (one fr. ounce) of diſtilled water, which diſſolved part of it. The ſolution was very alcaline.

The undiſſolved portion remained in a ſoft tenacious maſs, which adhered to the fingers when warm.

A larger quantity of water was added, and the whole was diſſolved.

Cauſtic potaſh does not facilitate its ſolution in cold water.

Cauſtic potaſh triturated for ſome time in a mortar with ambergris does not facilitate its ſolution in water.

Ammonia diſſolves it with the aid of heat.

Ammoniac does not act on ambergris cold, but when heated diſſolves it; the mixture gradually becomes brown, and on evaporation yields a glutinous ſaponaceous ſubſtance, in all reſpects ſimilar to that obtained by means of potaſh.

It is ſoluble in the fixed oils,

Exp. VII. The fixed oils, as thoſe of rape, olive, &c. diſſolve amber with the aſſiſtance of heat in a very ſhort time; the ſolution is yellow and tranſparent, and becomes brown on being evaporated.

and in the volatile oils.

Exp. VIII. Volatile oils likewiſe diſſolve ambergris.

Thoſe of turpentine, ſavine, and hyſſop, exhibit the ſame appearances. The ſolution aſſiſted by heat takes place pretty readily.

On evaporating the ſolution a red magma is left, which is ſoluble in alcohol.

On evaporation a thick red magma is produced, incapable of complete deſiccation, burning on the coals, and emitting a denſe ſmoak, of a ſmell reſembling that of the ambergris. Alcohol diſſolved this ſubſtance, and thence acquired a golden yellow colour, but it was precipitated from it by means of water.

Old volatile oils will not diſſolve it.

If volatile oils be too old, they will not completely diſſolve it, even with the help of long continued heat.

Soluble in ether.

Exp. IX. It diſſolves very quickly in ether, even cold.

Alcohol ſeparates its conſtituent parts.

Exp. X. The ſolution of ambergris by alcohol is the only one that is really capable of affording us any certain reſults. Its conſtituent parts may be ſeparated by it in ſuch a manner, that on reuniting them a compound is obtained, the qualities of which came very near thoſe of the original ſubſtance.

Part diſſolved in alcohol without heat;

3.821 grammes (one drachm) of ambergris were reduced to powder, put into a phial, and 61.143 grammes, (two ounces) of rectified alcohol were poured on them. A maceration of twenty four hours was ſufficient to give the alcohol a deep yellow colour; it was filtered, and a freſh quantity of alcohol was poured on the undiſſolved portion. The ſolution was facilitated by increaſing the temperature. The whole of the ambergris being diſſolved, except a ſmall quantity of black matter, the liquor was filtered while hot. It paſſed through the filter clear; but on cooling there ſeparated from it a light pale yellow ſubſtance, part of which adhered to the ſides of the veſſel.

another part by means of heat; leaving a little black matter; and ſeparating when cold.

The

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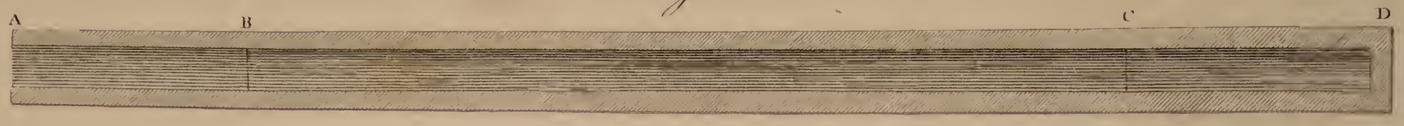
Fig. 1.



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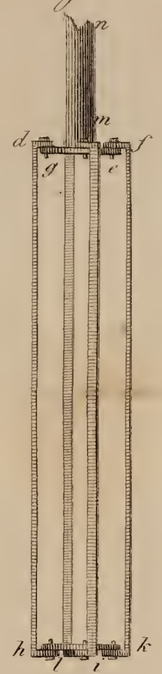

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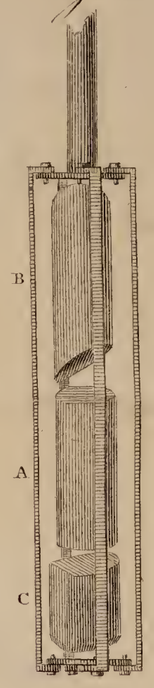

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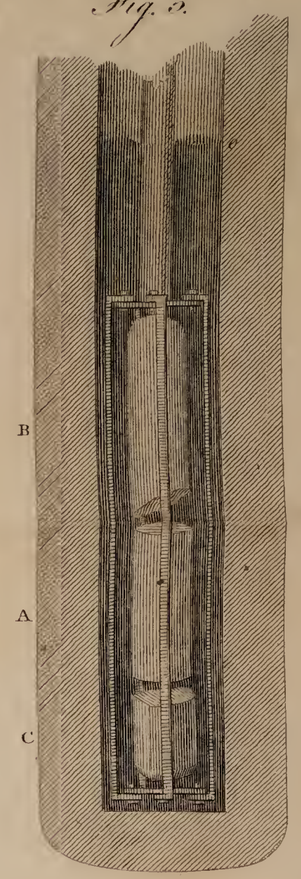

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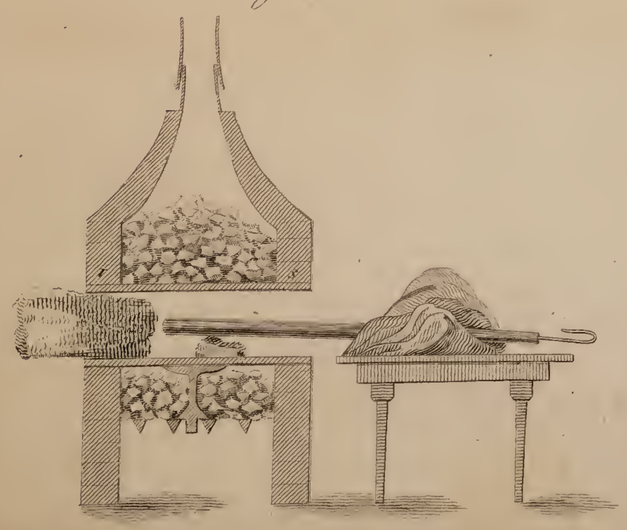

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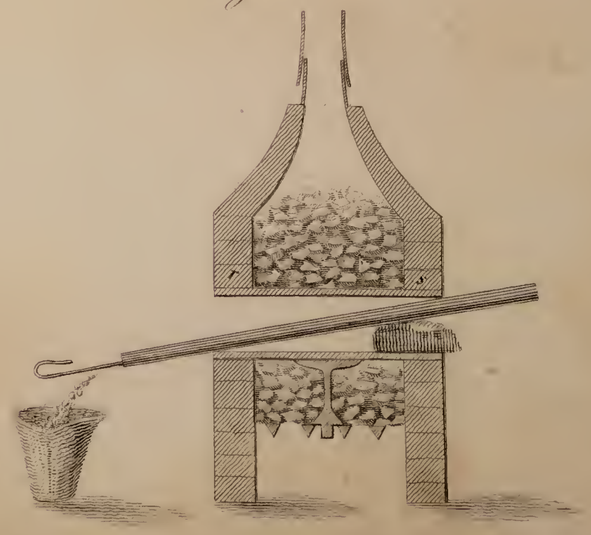

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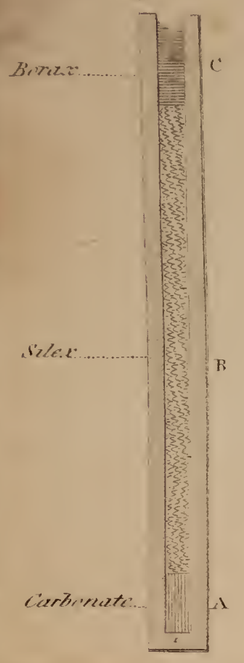

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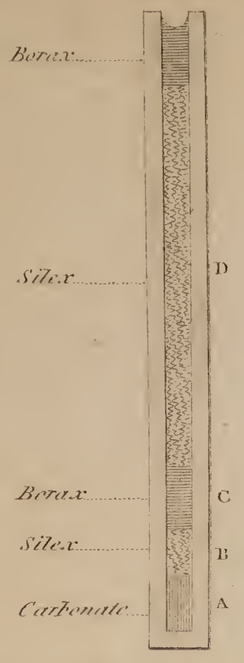

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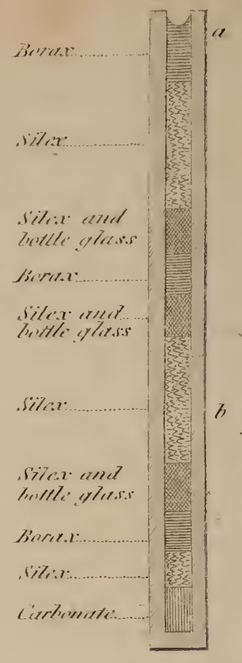

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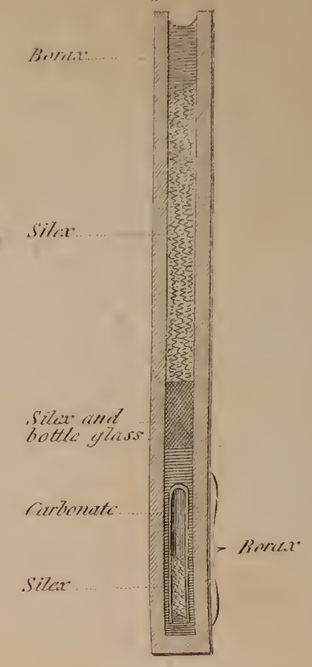

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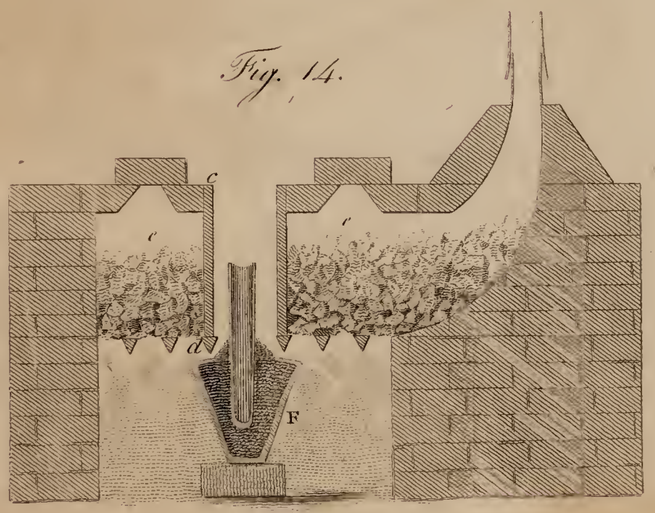

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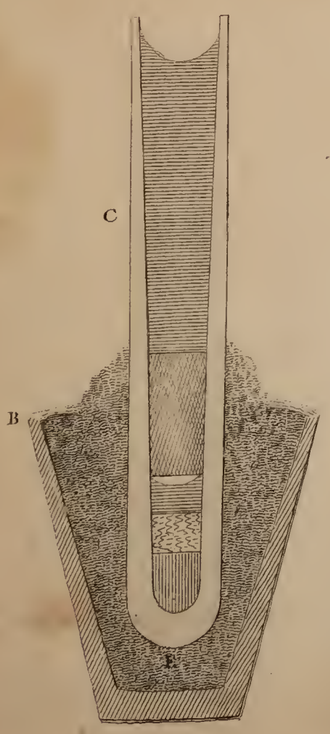

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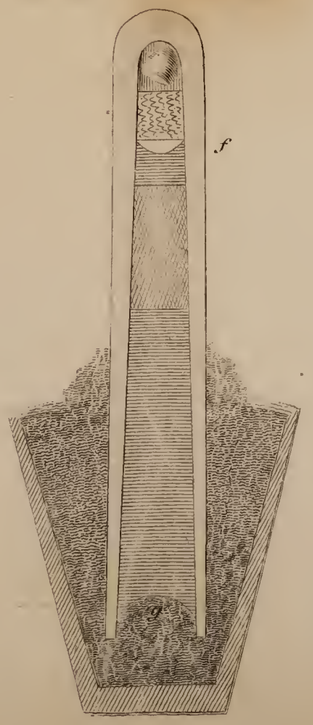

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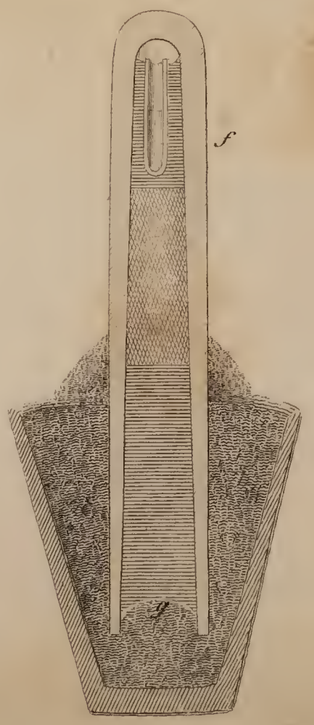

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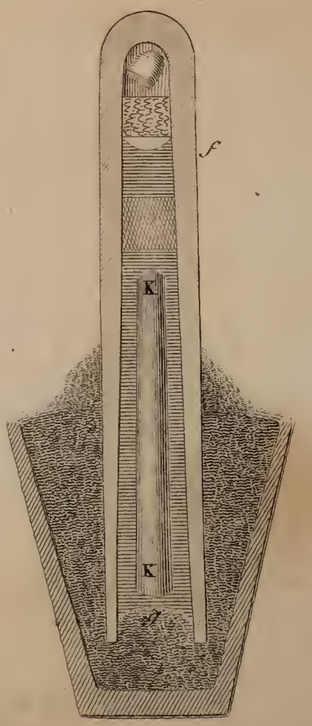

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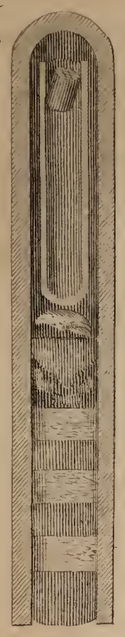

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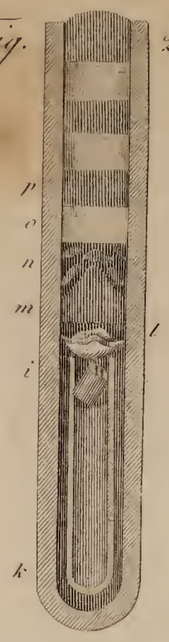

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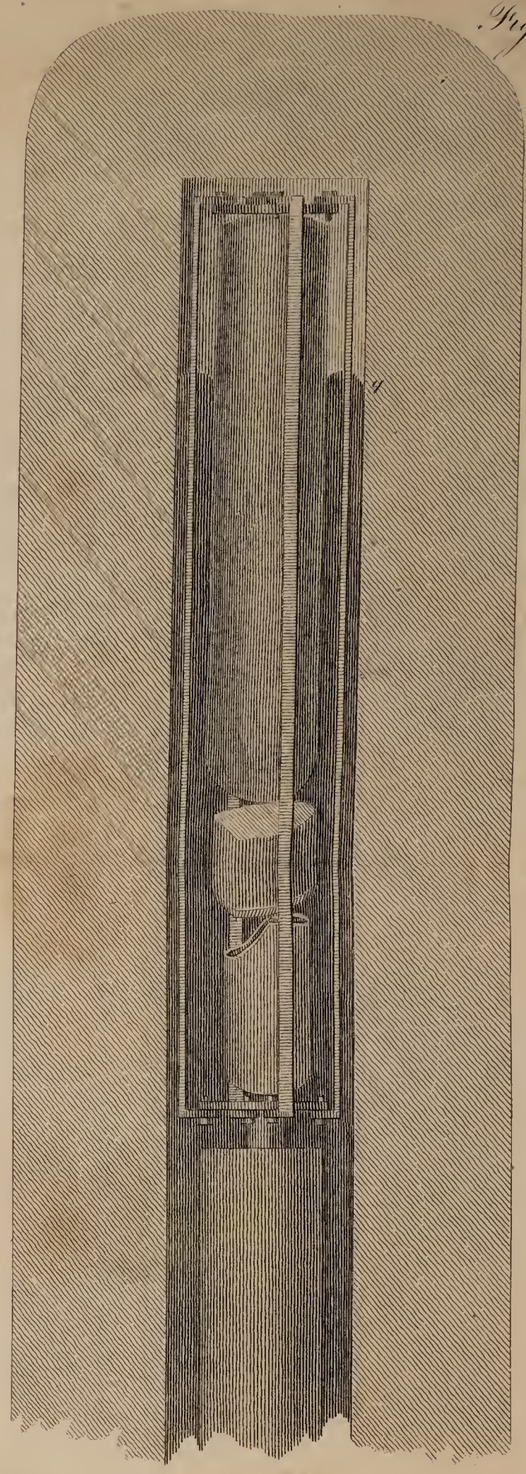

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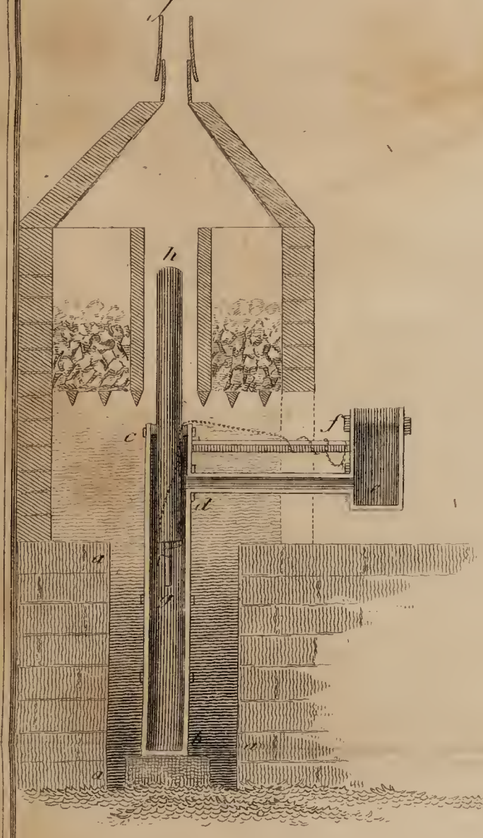

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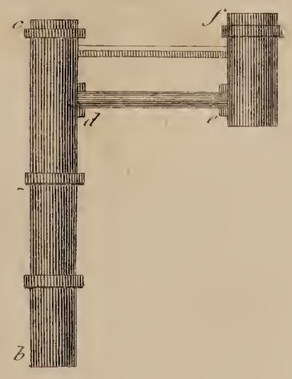

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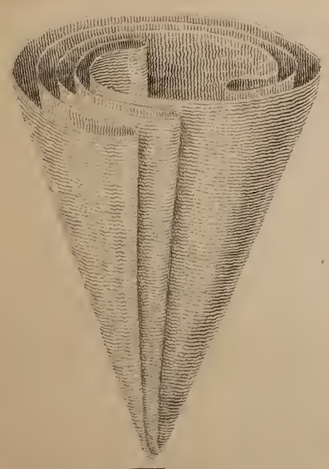

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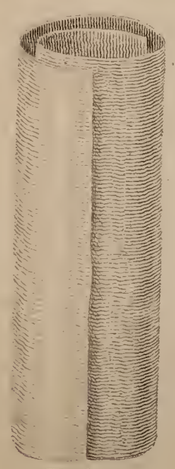

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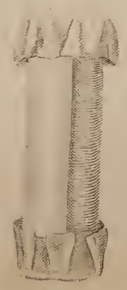

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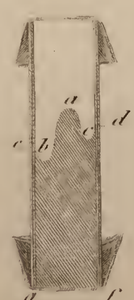
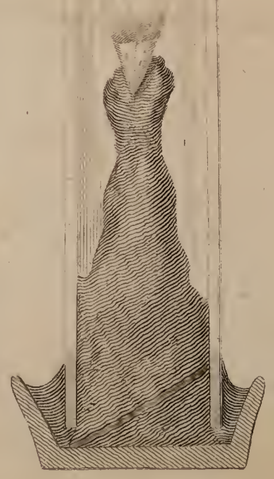


Fig. 32

Fig. 33



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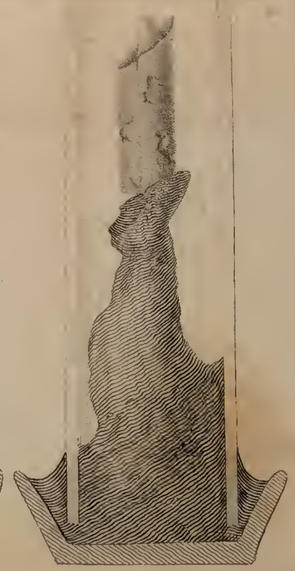

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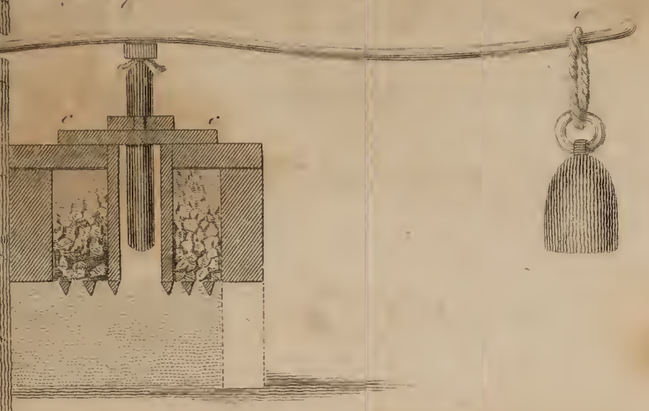

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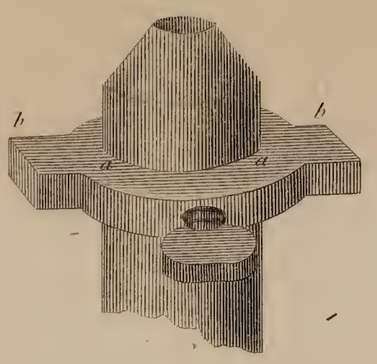

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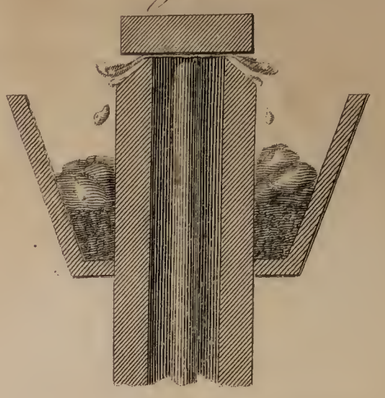

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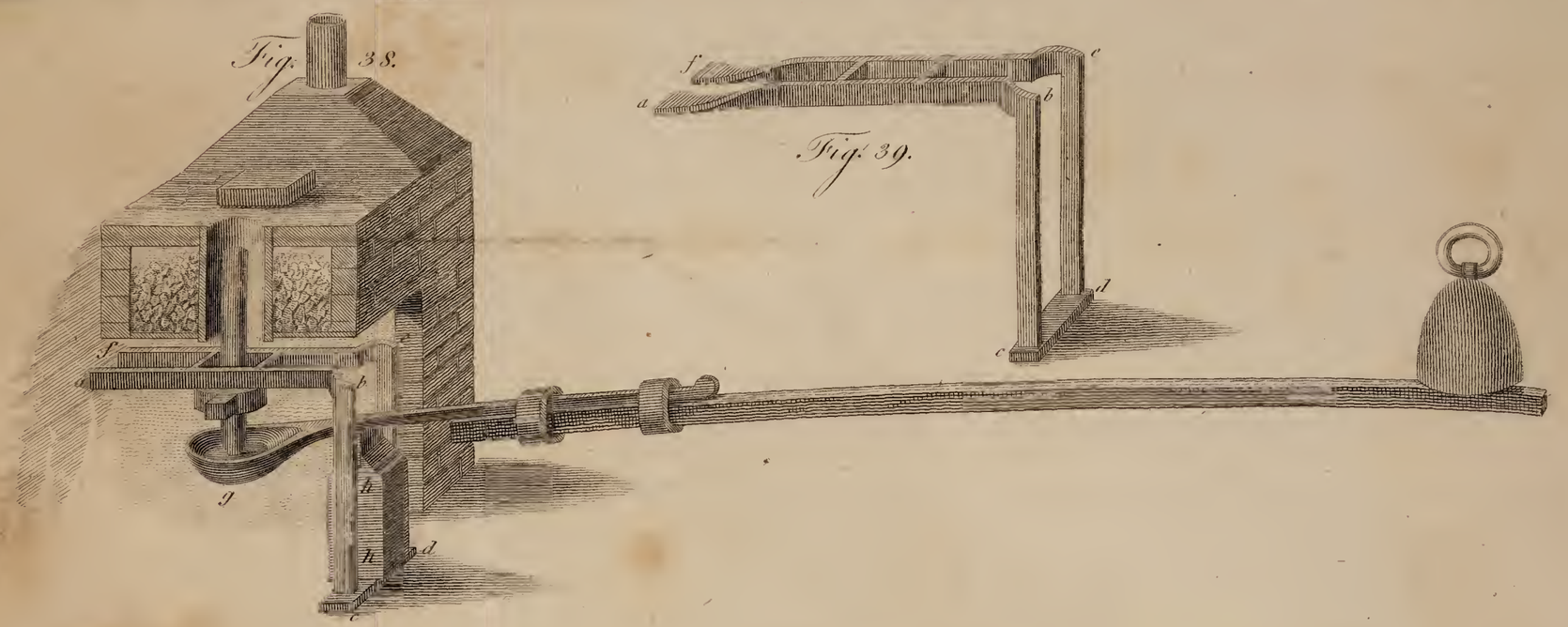

Fig. 39.

Fig. 40.

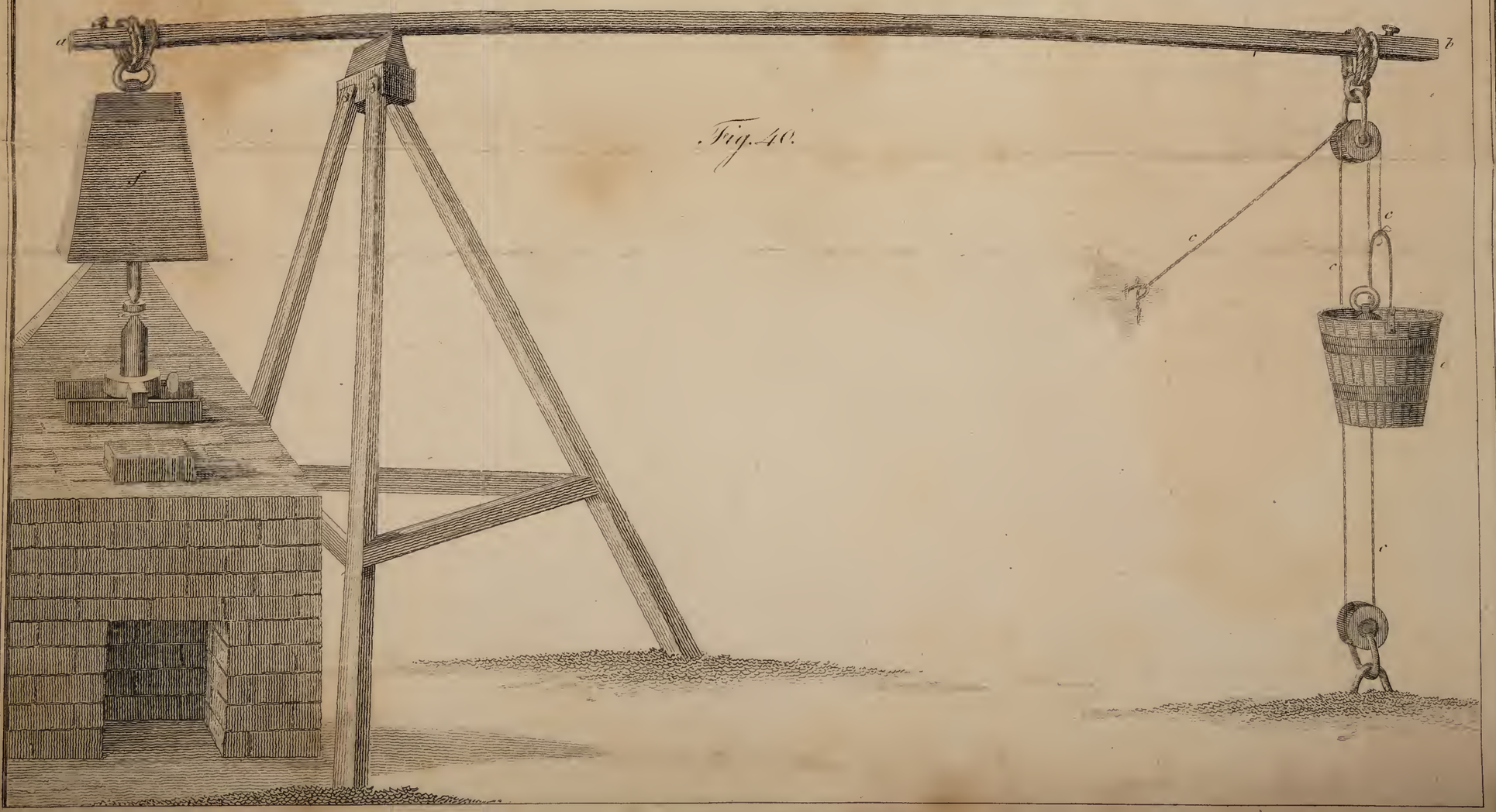

Fig. 41.

Fig. 42.

Fig. 43.

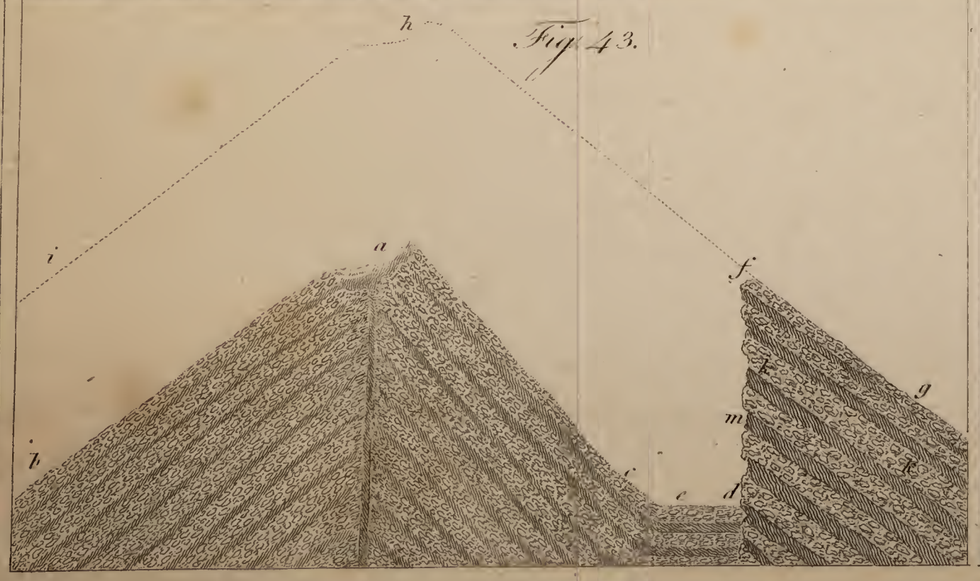

Fig. 44.

The firſt ſolution in alcohol made without heat, and that which was poured off from the precipitate, were mixed together, and evaporated to the conſiſtence of an extract: it was then of a reddiſh yellow colour, adhered to the fingers, had an agreeable ſmell, and a pleaſant taſte. The evaporation being continued to dryneſs, it appeared ſhining and tranſparent, grew ſoft between the fingers, and burnt in the ſame manner as reſins.

This ſolution mixed and evaporated.

left a reſinous ſubſtance.

The experiment was repeated, to determine the characters of theſe two ſubſtances more poſitively.

For this purpoſe ambergris was left to macerate in alcohol twenty-four hours as before; it was then filtered, and a freſh quantity of alcohol was added to the reſiduum, which was macerated in the ſame manner. The ſecond liquor was leſs coloured than the firſt. A third portion of alcohol being poured on what was left undiſſolved; its colour was ſcarcely altered. The ſlight action of the alcohol on this reſiduum ſeemed to indicate, that it was no farther ſoluble in this menſtrum; but I quickly found the contrary. I heated the mixture, and the whole was inſtantly diſſolved, leaving about 212 thouſandths of a gramme, (four grains) only of a black powder, which was nothing but oxide of carbone. The ſolution was filtered hot, and on cooling a whitiſh yellow glutinous ſubſtance was depoſited, which was ſeparated from the tincture.

The experiment repeated.

The black powder oxide of carbone.

This experiment ſhows us the poſſibility of ſeparating by means of alcohol three very diſtinct ſubſtances; the firſt ſoluble in it cold; the ſecond, by means of heat; and the third inſoluble, which remains in the form of powder.

Thus three different ſubſtances ſeparated.

To determine the characters of the firſt two ſubſtances, the tincture made without heat was firſt evaporated to dryneſs; when there remained in the capſule 1.167 grammes (22 grains) of a brown ſubſtance, dry and ſhining in its fracture, unalterable in the air, and growing ſoft with a gentle heat; 15° were ſufficient to give it a tenacious and glutinous conſiſtence; and being put on red-hot coals it was completely volatilized. If this experiment be made in a ſilver ſpoon, the volatilization takes place with the ſame rapidity, an odoriferous ſmell is diffuſed around, and no coally reſiduum is left.

The firſt examined.

Suſpecting this ſubſtance might be in ſome reſpect analogous to the reſin obtained from propolis by Cit. Vauquelin, I inſtituted a compariſon between them, and found the following differences:

Differs from the reſin obtained from propolis

1ſt.

in three reſpects.

1ſt. It melts much more ſlowly; 2dly, it diffuſes a denſe odoriferous vapour, reſembling a little the ſmell of honey; 3dly, it ſwells up, and leaves a very bulky coal.

Is a true reſin.

Finally, this firſt ſubſtance obtained from ambergris, which may be conſidered as a true reſin, is ſoluble in alcohol, and is precipitated by water. The ſolution reddens litmus paper, which proves too, that the alcohol diſſolves the benzoic acid previouſly detected, either by burning the ambergris under a bell, or by treating it with lime.

Examination of the ſecond ſubſtance.

Nothing now remains, but to examine the product obtained by heated alcohol, after the reſin is extracted by maceration.

I have ſaid above, that there ſeparated from the alcohol by refrigeration a ſubſtance, part of which ſubſided to the bottom of the veſſel, and part adhered to the ſides.

Being ſeparated from the liquor, and properly dried, it remains a little bulky and light. Under the preſſure of the finger it contracts and crumbles, but it is ſoon lengthened out and ſoftened by the heat. It has a laminated texture, if it be ſuffered to cool ſlowly.

It retains between its particles a little water and alcohol, which may be ſeparated by keeping it a ſhort time in fuſion. When melted over again it is much whiter than before, and no longer exhibits its former granulated texture. In fine, I have diſcerned in it all the properties of the adipocerous ſubſtance, diſcovered by Cit. Fourcroy in the fatty matter of dead bodies, and the properties of which he has deſcribed in a paper publiſhed in the 8th volume of the Annals of Chemiſtry.

Its properties the ſame with thoſe of the adipocerous ſubſtance found in the fatty matter of dead bodies.

From 3.821 grammes, (72 grains) of ambergris, 2.016 grammes, (38 grains) of adipocerous matter may be obtained.

Recapitulation.

From theſe experiments it appears we may conclude:

Recapitulation.

1ſt. That ambergris is a compound ſubſtance, which burns, and may be entirely volatilized.

2dly, that on diſtilling it alone we obtain from it a ſlightly acid liquor, and an oil partly ſoluble in alcohol, and of an empyreumatic ſmell.

3dly. That by ſublimation, or by the proceſs of Scheele, benzoic acid may be extracted from it.

4thly. That water does not act upon it.

5thly,

5thly. That by means of nitric acid a matter analogous to reſins, mixed with the adipocerous ſubſtance, is extracted from it.

6thly. That the concentrated ſulphuric, muriatic, and oxigenated muriatic acid, convert it to a coal, without diſſolving it.

7thly. That with alcalis it forms a ſaponaceous compound.

8thly. That fixed oils, volatile oils, ether and alcohol, are the true ſolvents of ambergris.

9thly. And laſtly, that alcohol affords the means of ſeparating its conſtituent parts in the following proportions.

Its conſtituent parts.

Adipocerous matter - - - -	2.016 grammes.
Reſin - - - - - - -	1.167
Benzoic acid - - - - -	0.425
Coally matter - - - - -	0.212
	3.820.

IV.

An Account of ſome Stones ſaid to have fallen on the Earth in France; and of a Lump of native Iron, ſaid to have fallen in India. By the Right Hon. CHARLES GREVILLE, *F. R. S**.

That ſtony and metallic bodies have fallen on the earth is fully eſtabliſhed.

THE experiments and obſervations made by Edward Howard, Eſq. on certain ſtony and metalline ſubſtances ſaid to have fallen on the earth, and the accurate deſcription which the Count de Bournon has given of thoſe ſubſtances, have, in my opinion, fully eſtabliſhed the following fact, namely, that a number of ſtones aſſerted to have fallen under ſimilar circumſtances, have preciſely the ſame characters.

The ſtones from Benares, that from Yorkſhire, that from Sienna, and that from Bohemia, were the whole which had then been ſeen in England. They all contained pyrites of a peculiar character: they all had a coating of black oxide of iron: they all contained an alloy of iron and nickel; and the earths which ſerved to them as a ſort of connecting medium, correſponded in their nature, and nearly in their proportions.

Three new ſpecimens from France.

Since the publication of Mr. Howard's and Count de Bournon's obſervations, I have received from France three

* From the Philoſ. Tranſ. 1803.

additional

Remarkable hiſtory.

additional ſpecimens. Monſieur St. Amand very obligingly divided with me a ſpecimen he had broken from a ſtone of about 15 inches diameter, preſerved in the Muſeum of Bourdeaux, which ſtone fell near Roqueford, in the Landes, on the 20th Auguſt, 1789, during the exploſion of a meteor; it broke through the roof of a cottage, and killed a herdſman and ſome cattle. M. St. Amand alſo gave me part of a ſtone he had preſerved in his collection ever ſince the year 1790, when a ſhower of ſtones, weighing from $\frac{1}{2}$ an ounce to 15 and 25 pounds each, fell in the pariſhes of Grange and Creon, and alſo in the pariſh of Juliac, in Armagnac; which fact was, at the time, verified by Duby, Mayor of Armile, and publiſhed by Bertholon, in the *Journal des Sciences utiles de Montpellier*, in the year 1790.

The third ſpecimen, I owe tothe Marquis de Dree; it is a fragment, broken from a ſtone of 22 pounds weight, which fell near the village of Salles, not far from Villefranche in Burgundy, on the 12th of March, 1798; this was alſo accompanied by a meteor.

Theſe three ſpecimens agree in characterwith the others.

I content myſelf with the mere recital of the facts, in confirmation of the obſervations preſented to the Society, as theſe three additional ſpecimens have preciſely the ſame characters, texture, and appearance, as the others in my collection; and are ſcarcely, by the eye, to be diſtinguiſhed from them.

I ſhould not, perhaps, have troubled the Society with this account, as my friend the Marquis de Dree, whoſe knowledge in mineralogy peculiarly qualifies him to inveſtigate theſe ſubjects, has given me hopes of ſeeing his obſervations on them publiſhed; but a new evidence has lately fallen into my hands, and is the only one I have met with that aſcertains the origin of native iron, which from analyſis, had been ſuſpected to have a common origin with the ſtones fallen on the earth. Converſing with Colonel Kirkpatrick, whoſe reſearches have embraced both the literature and politics of India, and whoſe talents had placed him in very important ſituations in various parts of India, I inquired whether he had ever heard of any inſtances ſimilar to the exploſion of the meteor at Benares in 1798. He told me, he could not recollect having heard or read of any other inſtance, excepting one in the Memoirs written by the Emperor Jehangire, and of that he did not recollect the particulars. A few days after, having found the

Metallic ſtone that fell in India nearly two centuries ago.

paſſage

paſſage in the original Perſian, he was ſo obliging as to tranſlate it. I conſider it as an authentic fact; for the Emperor Jehangire was not a prince on whom his courtiers would idly venture to impoſe; and there can be little probability that an Aumil of a diſtrict ſhould invent ſuch a ſtory, or be able to produce a ſubſtance apparently like iron, but which, on trial, differed from manufactured iron. Colonel Kirkpatrick's tranſlation I have obtained his leave to communicate, with his atteſtation, to the Royal Society.

Extract from the Memoirs of the Emperor Jehangire, written (in Perſian) by himſelf, and tranſlated by Colonel Kirkpatrick.

Narrative written by the Emperor Jehangire of a metallic ſtone that fell in the year 1620.

A. H. 1030, *or* 16*th year of the reign.*—The following is among the extraordinary occurrences of this period.

Early on the 30th of Furverdeen, of the preſent year *, and in the Eaſtern quarter, [of the heavens] there aroſe in one of the villages of the Purgunnah of Jalindher †, ſuch a great and tremendous noiſe as had nearly, by its dreadful nature, deprived the inhabitants of the place of their ſenſes. During this noiſe, a luminous body [was obſerved] to fall from above on the earth, ſuggeſting to the beholders the idea that the firmament was raining fire. In a ſhort time, the noiſe having ſubſided, and the inhabitants having recovered from their alarm, a courier was diſpatched [by them] to Mahommed Syeed, the Aumil ‡ of the aforeſaid Purgunnah, to advertiſe him of this event. The Aumil, inſtantly mounting, [his horſe,] proceeded to the ſpot, [where the luminous body had fallen]. Here he perceived the earth, to the extent of ten or twelve guz §, in length and breadth, to be burnt to ſuch a degree, that not the leaſt trace of verdure, or a blade of graſs remained; nor had the heat [which had been communicated to it] yet ſubſided entirely.

* The firſt of Furverdeen of this year, (A. H. 1030,) correſponded with Saturday, the 27th of Rubbi ul Akhir; conſequently, the 30th of Furverdeen fell on the 26th of Jummad ul Ouwul, or A. D. 1620.

† A purgunnah is a territorial diviſion, of arbitrary extent. The purgunnah of *Jalindher* is ſituated in the Punjaub, and about 100 miles S. E. of Lahore.

‡ Aumil is a manager or fiscal ſuperintendant of a diſtrict.

§ A guz is rather leſs than a yard.

Mahommed

Mahommed Syeed hereupon directed the aforeſaid ſpace of ground to be dug up; when, the deeper it was dug the greater was the heat of it found to be. At length, a lump of iron made its appearance, the heat of which was ſo violent, that one might have ſuppoſed it to have been taken from a furnace. After ſome time it became cold; when the Aumil conveyed it to his own habitation, from whence he afterwards diſpatched it, in a ſealed bag, to court.

It weighed upwards of four pounds, and was brittle.

Here I had [this ſubſtance] weighed in my preſence. Its weight was one hundred and ſixty tolahs.* I committed it to a ſkilful artiſan, with orders to make of it a ſabre, a knife, and a dagger. The workman [ſoon] reported, that the ſubſtance was *not malleable, but ſhivered into pieces under the hammer.*†

Upon this, I ordered it to be mixed with other iron. Conformably to my orders, three parts of the *iron of lightning*‡ were mixed with one part of common iron; and from the mixture were made two ſabres, one knife, and one dagger.

With the addition of one part of common iron to three parts of the metallic ſtone, excellent blades were made.

By the addition of the common iron, the [new] ſubſtance acquired a [fine] temper; the blade [fabricated from it] proving as elaſtic as the moſt genuine blades of Ulmanny,§ and of the South, and bending, like them, without leaving any mark of the bend. I had them tried in my preſence, and found them cut excellently; as well [indeed] as the beſt genuine ſabres. One of theſe ſabres I named *Katai*, or *the cutter*; and the other *Burk-ſeriſht*, or the *lightning-natured*.

A poet‖ compoſed and preſented to me, on this occaſion, the following tetraſtich.

" This earth has attained order and regularity through the
" Emperor Jehangire:
" In his time fell *raw* iron from lightning:
" That iron was, by his world-ſubduing authority,
" Converted into a dagger, a knife, and two ſabres."

* A tolah is about 180 grains, Troy weight.

† Literally, " it did not ſtand beneath the hammer, but fell to pieces.

‡ This expreſſion is equivalent to our term *thunder-bolt*.

§ The name of the place here deſigned is doubtful.

‖ The poet is named in the original; but the name is not perfectly legible.

The

The chronogram of this occurrence is contained in the words* ( ) which ſignify "the flame of the imperial lightning;" and give the year (of the Hegera) 1030.

N. B. The foregoing tranſlation (which is nearly literal) has been made from a manuſcript that has been ſeveral years in my poſſeſſion; and which, although without date, bears marks of having been written at a remote period.

WM. KIRKPATRICK.

V.

Analyſis of the Natrolite. By KLAPROTH.†

THE foſſil which forms the ſubject of this analyſis, and to which I give the name of *natrolite*, for reaſons to be ſtated hereafter, is found at Högau in Suabia, on the borders of Switzerland. It is depoſited in the crevices, or clefts and cavities of the ſonorous porphyry. (Klinginſtein Porphyr) from having a ſound, nearly metallic, which form the mountains and rocks of Hohentwiel, Hohenkrähen, and Mägdeberg. Natrolite found at Hogau, in Suabia.

The colour of this foſſil is a dirty ochreaceous yellow, approaching ſometimes to an Iſabella yellow, at other times to a yellowiſh brown, interſected with concentric white lines. It is compact, its internal fracture has a ſilky luſtre. It breaks into wedge-like pieces, the edges of which poſſeſs little tranſparency; it is not very hard, extremely brittle, and of=2,200 ſpecific gravity. Its colour a dirty yellow. Its properties.

A.

a. 100 grains of natrolite, after having been ignited for ſome time in a ſilver crucible, loſt nine grains. The figure of the ſtone was retained, but its compactneſs was conſiderably diminiſhed. Ignited in a ſilver crucible.

b. Before the blow-pipe on charcoal, natrolite fuſes quickly into a tranſparent glaſs, full of ſmall air bubbles. Fuſes quickly before the blow-pipe.

c. Natrolite placed in a clay crucible, and expoſed to the heat of a porcelain furnace, fuſed into a tranſparent glaſs of a light brown colour. In a porcelain furnace melts into a brown glaſs,

* The Perſian characters are given in the Tranſactions. N.

† Abſtract of an Eſſay in the memoirs of the Royal Academy of Sciences at Berlin, 1803, page 243.

d. In

with minute globules of iron in the ſurface.

d. In a charcoal crucible the mineral afforded the ſame product. The glaſs pearl exhibited on its ſurface minute globules of iron.

B.

Diſſolved in muriatic acid.

a. 100 grains of finely levigated natrolite, were mixed into a paſty fluid with water, put into a flaſk, and digeſted in moderately ſtrong muriatic acid. The ſolution was ſoon effected, and it exhibited a reddiſh yellow gelatinous maſs. After di-

The ſiliceous earth being precipitated by water,

luting it copiouſly with water, and continuing the digeſtion, the ſiliceous earth ſeparated, which, being collected and dried, weighed 48 grains.

cubic cryſtals were obtained.

b. The fluid obtained in the laſt proceſs on evaporation, yielded cubic cryſtals. The remaining fluid being further

The dry maſs digeſted in alcohol.

evaporated to dryneſs, the dry maſs was pulveriſed, and digeſted with a gentle heat in alcohol. Having ſuffered the alcoholic ſolution to cool, a white ſaline powder was depoſited; the alcohol was therefore decanted, and the powder collected, waſhed in ſpirit, and dried. The remaining alcoholic ſolution was afreſh evaporated, a ſmall quantity of the ſame ſaline powder became ſeparated, which was added to that obtained before.

The reſiduum diſſolved in water, and precipitated by ammonia.

c. The reſidue of the proceſs *b* inſoluble in alcohol, was diſſolved in water. On adding to this ſolution liquid ammonia, a light flocculent precipitate became depoſited. This being ſeparated by the filtre, the fluid which paſſed through, was evaporated by a gentle heat. The ſalt obtained, weighed when perfectly dry, $31\frac{1}{4}$ grains.

The ſolution in alcohol decompoſed by ammonia.

d. The alcoholic ſolution *b* (which from other experiments, was known already to contain nothing but alumine and oxide of iron) after being diluted with water, was decompoſed by liquid ammonia, and the precipitate collected and dried. The fluid, from which this precipitate had been ſeparated, was evaporated, and the maſs ſtrongly heated, ſo as to volatilize the muriate of ammonia that had been formed, when there remained two grains of ſalt, which being diſſolved in water, yielded cubical cryſtals.

The precipitates digeſted in ſolution of potaſh.

e. The precipitate obtained by means of liquid ammonia *d*, together with that before produced *c*, were put into a ſolution of pot-aſh, and digeſted with that fluid. A ſolution was effected, and oxide of iron ſeparated, which, after being ignited, weighed $1\frac{3}{4}$ grains.

f. The

f. The alcaline ſolution *e* was mingled with muriatic acid, till the precipitate produced, became rediſſolved, and was then decompoſed by carbonate of ſoda. The precipitate obtained after being waſhed, dried, and ignited, weighed 24¼ grains, It was alumine. Alumine ſeparated from the alcaline ſolution.

g. It remained ſtill to examine the alcaline part of the foſſil, which produced with muriatic acid the 31¼ gr. *c*. and the two grains *d*. Taſte, figure of cryſtals, and chemical reagents, proved it to be muriate of ſoda. A ſolution of it in water, mingled with a concentrated ſolution of tartareous acid, did not produce tartarite of potaſh. Another part of the ſolution, after being decompoſed by ſulphuric acid, yielded ſulphate of ſoda. The alcaline part of the foſſil, ſoda.

Having aſcertained by experiments that 100 parts of abſolutely pure carbonate of ſoda *, dried in a heat of ignition, when ſaturated with muriatic acid, loſt 41 parts by weight of carbonic acid, and yielded 120½ parts of dry muriate of ſoda (the deſiccation of which was not continued to decrepitation) we may conclude, that the above 33¾ grains of muriate of ſoda contained 16½ of ſoda.

100 parts of the natrolite conſequently yielded: Component parts of natrolite.

Siliceous earth	B.	*a*	48. grs.
Alumine		*f*	24.25
Oxide of iron		*e*	1.75
Soda		*g*	16.50
Water	A.	*a*	9.—
			99.50

The ſmall number of foſſils which contain ſoda, is therefore augmented by one more. That ſoda was contained in this ſtone might perhaps have been expected, on account of its forming frequently the matrix of the ſonorous porphyry, which,

* In order to obtain perfectly pure carbonate of ſoda, I diſſolve common carbonate of ſoda in water, and ſaturate this ſolution with nitric acid, taking care that the acid is a little in exceſs. I then ſeparate the ſulphuric acid by nitrate of barytes, and the muriatic acid by nitrate of ſilver. The fluid thus purified I evaporate to dryneſs, and fuſe the nitrate of ſoda obtained, and decompoſe it by detonation with charcoal. I then elixiviate the reſidue, filter, and cryſtalize the carbonate of ſoda. Pure carbonate of ſoda.

it is now known, always contains this alcali, but as the quantity of ſoda contained in this foſſil, is twice as large as that which exiſts in the ſonorous porphyry, I have not heſitated at giving it the name of *natrolite*.

VI.

*On the Employment of Aeroſtatic Machines in the Military Science, and for the Conſtruction of Geographical Plans.** *By* CITIZEN A. F. LOMET.†

Preſent ſtate of aeroſtation.

THE aeroſtatic art is ſtill in its infancy; and whatever progreſs may have been already made in it, it is impoſſible to foreſee all the reſources it may afford, or to determine the limits of its utility. Time and experience muſt fix our opinions reſpecting it; but it is of conſequence to obtain the aſſiſtance of learned men and artiſts in this intereſting purſuit; and as the ſmalleſt inveſtigations of this nature are generally too expenſive for individuals, it is neceſſary that the government ſhould ſupport an eſtabliſhment particularly devoted to the practice and improvement of the proceſſes which conſtitute it.

Its improvement ſhould be promoted by government.

Aeroſtats will furniſh, in preſence of an enemy, one or more points of obſervation at pleaſure, from which the poſitions he occupies may be reconnoitred, his movements ſtudied, and his manœuvres judged of in the groſs, or appreciated in the moſt minute detail. It may be preſumed that theſe machines will become of the moſt indiſpenſable utility in war, becauſe they ſupply it with an extraordinary means, hitherto unknown, of making obſervations, which may in an inſtant determine the fate of battles, ſecure the diſpoſitions for a vigorous defence, or at leaſt point out the moment and the moſt convenient outlets for a retreat; and more particularly to draw attention to the advantages which an army may derive from balloons, it will be ſufficient to remember the happy uſe made of them at the battle of Fleurus.

Advantages to be derived from its uſe in war.

Inſtance.

* Adjutant-commandant, formerly keeper of the collection of models belonging to the Polytechnic School, and now at the head of the ſixth diviſion of the war department. (Military operations and movement of the troops.)

† From Journal de l'Ecole Polytechnique, Tome IV. p. 252.

The

The Committee of Public Safety, and afterwards the Executive Directory, thought that the application of aeroſtats to military inquiries of every deſcription ought to be ſtudied and practiſed during peace. They were alſo deſirous, that they might be employed in the conſtruction of geographical plans, or at leaſt in aſcertaining the intermediate particulars of the territory between the points which had been geometrically determined. Having been charged with the experiments relative to theſe different applications, I purpoſe giving an account of the principal reſults.

Experiments undertaken by order of the French government.

The intention, from the firſt aſcents, was to meaſure the angle formed by the viſual rays falling on the eye of the aerial obſerver, from ſeveral determinate points on the earth. The unavoidable motion of the aeroſtat preventing the uſe of the graphometer in this operation, a recipiangle was at firſt ſubſtituted, ſuſpended like a mariner's compaſs, by the aſſiſtance of which, it was hoped the meaſure of the angles would be eaſily taken, and particularly that they would be obtained with immediate relation to a horizontal plane. This attempt not having ſucceeded, it was neceſſary in future to make uſe of a ſextant.

Firſt attempts at meaſuring the angle of the viſual rays.

This inſtrument was every thing that could be deſired for celerity, as well as for the facility and preciſion of the obſervations, but it has this inconvenience, in the caſe in hand, that it only ſhews the angle on a plane inclined to the horizon; and moreover, in its ordinary conſtruction it furniſhes no means of noticing this inclination. The perpetual agitation of the machine is another ſource of error; in fact, an aeroſtat, kept elevated and held by cords, is continually changing poſition; it moves in ſpace, deſcribing alternate ellipſes, the curvature of which is modified to infinity, according to the violence of the wind, the elaſticity of the cords, and the ſituation of the places to which it is faſtened. It leaves then no trace of its variations, and does not permit the obſerver which it ſupports, to add to the meaſure of any angle whatſoever, that of the two angles neceſſary to connect the firſt with the plane of the horizon.

The uſe of the ſextant is not free from error.

Nevertheleſs, for plans relative to the generality of military inquiries, and in all caſes where a ſketch of the figure of the earth is ſufficient without attending to ſlight inaccuracies in diſtances, ſimple obſervations, made with the ſextant, will anſwer the purpoſe, and furniſh the means of operating with

But is nevertheleſs applicable in many caſes.

facility over a vaſt extent of territory, ſecure from the attempts of an enemy. But it is not equally ſerviceable in operations which require a rigorous exactneſs, and in which it is requiſite to connect the angles with the centre of the ſtation, and with the plan of the horizon.

Additional apparatus to render the ſextant capable of indicating *all* the required angles.

The following is the mode in which I have endeavoured to fulfil the various objects:

The angles neceſſary for connecting the poſition of two objects with the centre of the ſtation and the plane of the horizon, are, 1ſt. the angle comprehended between the rays falling on the eye of the obſerver from theſe points; 2d. the angles formed by each of theſe rays with the perpendicular.—We have ſeen, that it is impoſſible for the aeroſtatic obſerver to mark theſe three angles by taking them after each other; but if their meaſure were inſtantaneous, the difficulty would be overcome. This would therefore be the caſe if an inſtrument could be deviſed, which would give theſe three angles at once by a ſingle obſervation; and as the ſextant already ſhows the angle comprehended between the rays, the object in view is to add the neceſſary parts to that inſtrument for obtaining the other two at the ſame time.

Preliminary obſervations.

Let B A C *(Plate* X.) be the angle formed by the viſual rays A B, A C; falling on the mirror A of the ſextant from the objects B and C; if the index A D be moved until the image of the object C reflected by the mirror A placed on the index coincides by double reflection on the mirror L, with the point where the object B is ſeen; and if they be both perceived at the ſame time by the obſerver looking through the teleſcope P, it is known, *(by the Theory and Uſe of the Sextant)* 1ſt, That the angle D A E, compriſed between the index A D and the fixed radius or line of zero A E of the inſtrument, is always equal half the angle B A C, the meaſure of which is required: 2d, That the line R B, which is ſuppoſed to paſs through the axis of the teleſcope and the centre of the mirror L, is always directed to the point B, and is uſually taken for the ſide A B; the error ariſing from the ſmall diſtance A R being conſidered as nothing in practice: hence, if we ſuppoſe a viſual ray paſſing from the point R to the object C, the angles B R C and B A C may be reputed equal, and be taken indiſcriminately for each other.

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This being premiſed, if a ruler be placed in the direction R B, it may be conſidered as in that of the ſide A B, and if we can ſucceed in fixing a ſecond ruler in ſuch a manner that the moveable index ſhall carry it into the direction R C, at the inſtant that the images of the two objects B and C are brought into one at the point L, it is evident that theſe two rulers will form between them the angle B R C, and conſequently the angle B A C. Method of aſcertaining the angle contained by two viſual rays.

To accompliſh this, let us ſuppoſe a ſort of falſe ſquare, S R Q, ſituate in the plane of the inſtrument, and moveable at its axis on a pivot fixed at the point R, at the interſection of the lines A R and R L; making the angle S R Q, compriſed between its arms, equal to the angle E R L, and the ſide R S equal to the diſtance A R. If now we ſuppoſe that the extremity S of the ſide R S, is retained by a button in a groove M N, worked in the moveable index, the movement of it will be communicated to the falſe ſquare in ſuch a manner that the angle L R Q will always be equal to the angle B A C, and conſequently the ſide R Q will be placed in the requiſite direction.

In fact, the triangle A R S being iſoſceles in its form, the exterior angle S R E = R A S + A S R. = 2 R A S = B A C; but S R Q being equal to E R L, if the common angle S R L be taken away, there will remain the angle S R E = L R Q = B A C.

Now let us fix under each of the two rulers R L and Q R a ſmall graduated quadrant, ſuſpended in ſuch a manner that it will place itſelf in the vertical plane of the ſide correſponding to the angle obſerved; let us affix to each of theſe quadrants a plummet, compoſed of a ſtiff arm moveable upon a pivot, and furniſhed with a nonius index and a weight, which gives it a conſtant tendency to aſſume a vertical poſition, in whatſoever ſituation the ſextant may be placed; finally, let the whole be ſo diſpoſed that the index of each plummet may be retained at will, at the diviſion indicated on the limb by the effect of the ſuſpenſion, and this by means of a trigger, which can be pulled at the exact inſtant of obſerving the principal angle in the points of reflexion. It is evident that the ſimultaneous action of the two rulers and the plummets will ſhow the three angles ſought, and that nothing remains but to reduce, by calculation, the angle B A C to the plane of the horizon. Obſervation of the angle formed by the viſual rays and the perpendicular. From which theſe on the plane on the horizon are obtained by calculation.

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Succeſs of the inſtrument.

This inſtrument, arranged in the manner here deſcribed, produced every deſired effect in our experiments. The invention, as ſimple as it is happy, may become very important from the uſeful applications of which it is ſuſceptible; and there is already reaſon to hope that, by bringing this to perfection, or by the formation of ſome analogous inſtrument hereafter, there will be a poſſibility of executing trigonometric operations with much correctneſs, by the aſſiſtance of aeroſtatic machines, notwithſtanding their continual motion.

Probable future advantages.

Method of connecting all the obſerved angles in one common centre.

It was not enough to have diſcovered the means of connecting the angles with the plane of the horizon: it was ſtill deſirable, that all the angles obſerved during aſcents in any one place, ſhould have a relation to the common centre of obſervation. To accompliſh this, it was neceſſary to keep a regiſter, by ſome means, of the ſituation of the machine at the preciſe moment of meaſuring each of theſe angles. This was done by dropping from the aeroſtat, at that inſtant, a ſmall ſtake, leaded and furniſhed with an iron point. This ſtake fell rapidly to the earth, into which it ſtuck, and marked a point correſponding to the ſummit of the angle meaſured. It was then eaſy to compare the poſition of this point, with that of one taken for the common centre of the obſervation, and thence to deduce the neceſſary corrections. It muſt however be noticed, that the ſtake, when abandoned to itſelf, acquires, at the inſtant of its fall, a compound motion which partakes of that of the aeroſtat, and conſequently is not exactly vertical; but the error which reſults from this deviation is but ſlightly perceptible in practice.

Application of the proceſs.

The calculations and ordinary proceſſes of deſcriptive geometry will furniſh all the means of making uſe of theſe different obſervations, and of expreſſing the reſults on paper; not only for their application to the conſtruction of maps, but alſo to aſcertain heights compared with the level: but we ſhall not in this place enter into any details on that head.

Inaccuracy to be expected from an inexperienced obſerver.

The obſerver engaged making theſe firſt experiments, ſoon perceives that the involuntary embarraſſment, occaſioned by the novelty of his ſituation, when he finds himſelf inſulated and ſuſpended at a height of ſeven or eight hundred metres, has a conſiderable influence both on the fidelity of his obſervations and on the time neceſſary for making them. All certainty depends, in fact, upon the confidence and readineſs

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of the obſerver; and it cannot be concealed, that it may produce great inconveniences, becauſe this difficulty of operating opens wide limits for the errors which it is poſſible to commit.

Conditions neceſſary to make correct obſervations.

From this laſt obſervation it will be ſeen: 1ſt. That it is indiſpenſably neceſſary to have acquired a great aptitude for theſe ſort of obſervations, to be able to execute them with preciſion: 2d. That the proceſſes juſt deſcribed are more ſatisfactory in theory than they would be in thoſe applications which require ſtrict accuracy; and, that though there are ſituations in which nothing can be ſubſtituted for their uſe in the conſtruction of ſome figured plans, it is at leaſt proper never to uſe them in preference to thoſe means of obſervation which are better known, and which can be employed with more certainty.

Advantages to be derived by employing balloons in ſurveying countries.

But it cannot be too often repeated, at the ſame time, that aeroſtats furniſh the means of giving the moſt lively intereſt to the delineation of the figure of the earth, in maps of all deſcriptions; and that their uſe is of the greateſt aſſiſtance in the formation of plans, the outline of which may be ſufficiently defined by a ſimple eye-ſketch. The aerial obſerver, by diſcovering a vaſt extent of country, accuſtoms himſelf fully to conſider the general organiſation of the aſperities of the country, and even the particulars of its varieties, as well as the tone of colour, which appear to give a different character to each portion of territory. If this new method of obſervation be cultivated with aſſiduity, it will doubtleſs lead to a ſenſible improvement in the art of drawing plans. But to benefit by the advantages which it affords, it is neceſſary, that thoſe who are deſtined to this employment ſhould join a profound knowledge of geometry to a great facility in deſigning landſcapes. May they be convinced of the importance of this truth, and aſſure themſelves that no part of the plan can arrive at perfection, either ornamental or relative to civil and military purpoſes, unleſs ſtrict accuracy in the outline is accompanied by that fidelity of expreſſion which is capable of producing in thoſe who inſpect the plans, all the ideas which the obſerver had formed from the aſpect of the country.

Valuable properties poſſeſſed by theſe machines.

From all that has been ſaid, we may conclude, that the aeroſtatic art combines properties no leſs valuable than unqueſtionable in topographic operations and military reſearches; that its perfection

fection may produce new and invaluable properties; and that it would be equally impolitic to neglect the uſe of theſe machines, or not to obtain for them the information to be derived from reflection and experience. We ſhall terminate this memoir by an obſervation relative to their military uſes. Our enemies would not fail to oppoſe to the creative induſtry of of France, an induſtry of imitation: they would alſo have their balloons and ballooneers *(aeroſtiers.)* The influence of this innovation in war is of a nature to ſpread with rapidity, and it muſt ſoon ceaſe to favour any nation excluſively. But even in this caſe the art of aeroſtatic machines will have acquired a higher degree of intereſt, becauſe another element ſhall then be in the power of man, in which the efforts of genius and induſtry may be ſubſtituted inſtead of the inconſiderate devaſtations of force; and this obſervation ought to intereſt the friends of hu-manity in bringing them to perfection.

Military uſe of them.

VII.

Chemical Analyſis and Properties of Arſeniated Hidrogen Gas
By Professor Tromsdorff.*

Scheele diſcovered an inflammable arſeniated gas.

THE immortal Scheele, in his eſſay on arſenic and arſenic acid,† mentions an inflammable arſeniated gazeous fluid, of which he ſays: "*Hinc intelligas, hunc aerem inflammabilem eſſe, regulumque arſenici ſolutum tenere.*" Scheele ſtates, that he obtained this gas during the ſolution of tin in arſenic acid. The properties of this gas, as pointed out by him, are the following. Arſeniated hidrogen gas is inſoluble in water; it does not render lime-water turbid; mingled with atmoſpheric air, no diminution of bulk enſues; on bringing the flame of a taper in contact with this mixture, a loud detonation follows, and metallic arſenic is depoſited. Intereſting as the obſervations here pointed out muſt appear to every chemiſt, the object has been neglected by ſucceeding operators.

Its properties.

* From a memoir, read in the Royal Academy of Sciences at Berlin, 1803, p. 370.

† C. H. Scheele Om Arſenick och deſs Syra; Kongl. Svenſk. Vetenſkaps Academiens *Handlingar*. Ar. 1775. V. xxxvi. 265.

Prouſt

Prouſt is the only philoſopher who mentions this gas: * he obtained it by digeſting arſenious acid and zinc, in dilute ſulphuric acid; on burning the gas, he obtained ſometimes arſenious, at others arſenic acid. Being perſuaded that the formation and properties of this gas deſerved a cloſer examination, I inſtituted a ſeries of experiments, the reſults of which are as follows.

Prouſt mentions it.

Methods of obtaining arſeniated Hidrogen Gas.

1. There are a variety of proceſſes for obtaining arſeniated hidrogen. It is produced by heating tin filings in liquid arſenic acid. This method is the moſt expenſive and moſt tedious. During the evolution of the gas in this proceſs, arſenic, alloyed with tin, is precipitated, and the fluid obtained, holds in ſolution, arſeniate of tin.

Proceſſes for obtaining arſeniated hidrogen gas.

2. It is likewiſe formed by treating in a ſimilar manner, arſenic and iron with muriatic acid.

3. Arſeniated hidrogen is alſo produced by heating a mixture of arſenious acid, iron filings and muriatic acid. The fluid, in this caſe, contains muriate of iron and muriate of arſenic.

4. Tin filings and arſenic acid yield this gas under ſimilar circumſtances.

5. Four parts of granulated zinc and one of arſenic, treated in a ſimilar manner with ſulphuric acid, previouſly diluted with two parts of water, afford arſeniated hidrogen very readily.

The gas obtained according to either of theſe proceſſes, is nearly alike, but that produced according to the laſt proceſs ſeems to be the moſt perfect gas, for it contains no exceſs of hidrogen. When arſeniated hidrogen is produced by means of zinc, arſenic, and dilute of ſulphuric acid, the quantity of arſeniated hidrogen is leſs than the quantity of hidrogen which would be obtained in decompoſing water in a ſimilar manner, without the interpoſition of arſenic. The reſidue, after the evolution of the gas has ceaſed, contains metallic arſenic; part of the hidrogen muſt therefore have acted on the oxigen of the arſenic acid, in order to reduce it to the metallic ſtate. From what has been ſtated, it appears that arſeniated hidrogen contains arſenic in a metallic ſtate, and not in the ſtate of arſenious, or arſenic acid. This will become more obvious in the ſequel of this paper.

Beſt produced from zinc, arſenic, and ſulphuric acid.

* Journ. de Phys. T. II. p. 173.

Phyſical

Phyſical Properties of Arſeniated Hidrogen Gas.

Its phyſical properties.

Arſeniated hidrogen is a permanent elaſtic aeriform inviſible fluid. It is a true chemical compound. Prouſt aſſerts that it depoſits arſenic: This however I have never been able to obſerve, if the gas were *pure*. It has an alliaceous fetid ſmell. It extinguiſhes burning bodies. It is not abſorbable by water; but when this fluid is freed from atmoſpheric air, it takes up a ſmall quantity of the gas which becomes diſengaged again by mere agitation. It does not change the colour of tincture of litmus. The ſpecific gravity of arſeniated hidrogen is, at 28′ barometrical preſſure = 0,5293, or, one cubic inch (old French meaſure) weighs 0,2435 grains. It is therefore lighter than oxigen, nitrogen, atmoſpheric air, carbonic acid, nitrous gas, ammonia, and gazeous oxide of carbon, but heavier than hidrogen and ſulphurated hidrogen gaſes. It is abſolutely fatal to animal life.

Chemical Properties of Arſeniated Hidrogen Gas.

Its chemical properties.

Arſeniated hidrogen, mingled with atmoſpheric air, ſuffers no chemical change, but mere dilution. The ſame holds good with reſpect to nitrogen. When mingled with nitrous gas, a diminution of 0,02, or 0,03 takes place, which ſometimes even amounts to 0,05. To aſcertain the nature of this gas, I mixed two parts of arſeniated hidrogen, with one of nitrous gas, and gradually added oxigen, till no further diminution of bulk enſued. On preſenting to this mixture a lighted taper, a loud exploſion took place, accompanied with flame. Probably part of the oxigen added, remained uncombined; for a mixture of two parts of nitrous gas, and three of arſeniated hidrogen, could not be inflamed by the taper; arſeniated hidrogen is miſceable with hidrogen, with carbonic acid, and with ammonia in all proportions.

Mixed with nitrous gas.

With oxiginized muriatic acid gas.

Into a cylinder half filled with arſenic and hidrogen, I ſent up bubbles of oxiginized muriatic acid gas. The bulk of the gas was diminiſhed, heat was evolved, and metallic arſenic was depoſited in a cryſtalline ſtate. On adding to the mixture an additional doſe of oxiginized muriatic acid gas, white fumes appeared, and the depoſited metal vaniſhed. The ſame experiment was repeated ſucceſſively, taking care to add no more of the latter gas, than was juſt ſufficient to occaſion the precipitation

cipitation of metallic arſenic. The collected metal yielded nitrous gas, by the affuſion of nitric acid, and on adding to this mixture muriatic acid, arſenic acid was produced. The arſenic depoſited in the manner ſtated before, when laid on ignited coals, became volatilized in thick white fumes, yielding arſenious acid. The precipitation of metallic arſenic muſt be aſcribed to the decompoſition of the oxiginized muriatic acid gas; the oxigen of this gas uniting with part of the hidrogen of the arſeniated hidrogen, and forming water, and thus ſeparating the arſenic. For the arſenic is capable of being oxided by the muriatic acid. Should it be imagined, that arſenic exiſted in arſeniated hidrogen, in the oxidized ſtate, and that it became precipitated by the oxiginized muriatic acid robbing it of its oxigen, we ſuppoſe things analogically erroneous, for the oxiginized muriatic acid is more capable of giving out oxigen than of taking it. The experiments of Chenevix ſeems perhaps hoſtile to this aſſertion; but the experiments of this philoſopher merely prove that the oxiginized muriatic acid is capable of combining with an additional doſe of oxigen, and conſtituting with it a hyperoxiginized muriatic acid. This certainly cannot be the caſe in the preſent inſtance, as will appear more evident from what I ſhall ſtate preſently.

Further experiments with this gas over quickſilver.

I filled a cylinder in the mercurial pneumatic trough, with arſeniated hidrogen, and ſent up into it as expeditiouſly as poſſible, a quantity of oxiginized muriatic acid gas. The reſult was evolution of heat, diminution of volume, and the inner ſides of the cylinder became covered with a kind of dew. A formation of water had therefore actually taken place in this experiment. Into another dry cylinder half filled over mercury, with arſeniated hidrogen, I introduced dry muriatic acid gas. In this caſe no diminution of bulk, no ſeparation of arſenic enſued; no change at all took place. Repeating the ſame experiment, I introduced into the cylinder a ſmall quantity of water; the muriatic acid gas was abſorbed, and the reſidue was arſeniated hidrogen unaltered.

Into a cylinder half filled with oxiginized muriatic acid gas, I paſſed gradually arſeniated hidrogen in ſmall bubbles at a time; in this caſe no metallic arſenic was ſeparated, but thick white clouds appeared. On continuing the addition of arſeniated hidrogen till no more white fumes appeared, metallic arſenic was depoſited. It follows from this experiment, that

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when a *ſmall* quantity of arſeniated hidrogen is made to act upon a *large* quantity of oxiginized muriatic acid gas, part of the oxigen of the oxiginized muriatic acid gas combines, not only with the hidrogen of the arſeniated gas, and forms water, but the metallic arſenic alſo becomes oxidiſed. Reaſoning from this fact, we ſhould be inclined to believe, that a mutual decompoſition of both the gaſes could be thus effected; but this cannot be accompliſhed; a diminution of bulk indeed takes place to a certain extent, but the complete diſappearance of both the gaſes cannot be effected. If the admixture of arſeniated hidrogen, with this oxiginized muriatic acid gas, be continued no longer than white clouds appear, and the reſidue be then examined, it will be found to conſiſt of hidrogen and oxiginized muriatic acid gaſes; and the mixture detonates at the approach of a taper. The oxiginized muriatic acid gas can only be ſeparated with difficulty by long agitation, in contact with water, and it ſeems as if it were become leſs ſoluble in that fluid. If the ſeparation of this gas be accompliſhed, the remaining arſeniated hidrogen burns with a pure flame, void of alliaceous odour, and contains no veſtige of arſenic, as ſhall be proved hereafter. From what has been ſtated, the following theory may be formed.

Theory of the decompoſition of arſeniated hidrogen gas.

Arſenic, in combination with a certain portion of hidrogen, conſtitutes arſeniated hidrogen gas. On preſenting to this combination oxiginized muriatic acid gas, the oxigen of this gas combines with the hidrogen, which held in ſolution the arſenic, and the latter is ſeparated. If more oxiginized muriatic acid be added than is neceſſary for this purpoſe, the portion of oxiginized muriatic acid gas does not act further upon the hidrogen, but merely upon the arſenic, and the latter becomes oxidiſed.

Arſeniated hidrogen gas mixed with hidrogen.

Hidrogen and arſeniated hidrogen may be mingled without decompoſing each other; the decompoſition can only be effected by the contact of fire; but if we mingle hidrogen, holding in ſolution ſulphur and oxiginized muriatic acid gas, the decompoſition and formation of water is inſtantly effected. This is likewiſe the caſe with arſeniated hidrogen gas.

Hidrogen combined with metallic ſubſtances.

Hitherto no combination of hidrogen with a metallic ſubſtance has been known; but it is highly probable, that ſuch combinations may exiſt. This indeed ſeems to be the caſe in the formation of this gas on which we are treating. If this be admitted,

mitted, a diviſion of hidrogen muſt take place, in the following manner; one part of it muſt unite with the oxigen of the oxiginized muriatic acid gas, to produce water; another part muſt fall down with the arſenic; and another portion remains combined with caloric, in the form of hidrogen gas.

Sulphurated and arſeniated hidrogen gaſes.

Hydrothian acid gas * and arſeniated hidrogen do not act upon each other. To a mixture of equal parts of hydrothian acid gas, and arſeniated hidrogen gas, I added gradually oxiginized muriatic acid gas; a diminution of volume inſtantly took place, accompanied with liberation of heat, and a depoſition of yellow ſulphurized arſenic (orpiment). On adding an additional quantity of gas, the precipitate acquired a beautiful orange red colour, and on continuing the addition of oxiginized muriatic acid gas, white clouds were produced, the precipitate detached itſelf from the ſides of the veſſel, and were gradually converted into a pulverulent ſubſtance of a yellowiſh white colour.

Oxiginized muriatic acid gas added to theſe.

Teſt of arſeniated hidrogen.

The reſults of theſe experiments are obvious, and might have been expected *a priori*. But they may ſerve as a teſt to diſcover the preſence of arſeniated hidrogen, when contained in other gaſes.

Nitrogen mixed with it.

I mingled one cubic inch of arſeniated hidrogen with ten of nitrogen, and one of hydrothian acid (ſulphurated hidrogen gas;) on adding to this mixture a ſmall quantity of oxiginized muriatic acid gas, yellow ſulphurized arſenic was inſtantly depoſited. It is not improbable, that arſenic is likewiſe ſoluble in other gaſes, and in this caſe the hydrothian acid (liquid ſulphurated hidrogen,) in conjunction with oxiginized muriatic acid, would prove a uſeful re-agent for diſcovering the preſence of it.

Extinguiſhes a lighted candle, but inflames.

A lighted taper immerſed in a vial filled with arſeniated hidrogen, is inſtantly extinguiſhed; at the ſame time that the gas burns at the orifice of the vial with a lambent white flame, diffuſing a diſagreeable odour, and much white fumes, which are arſenious acid. If the gas be inflamed in a phial with a ſmall orifice, the flame deſcends gradually down to the bottom of the phial, which becomes lined with a coat of cryſtallized metallic arſenic. In this caſe therefore the hidrogen alone burns.

* The name given by the Germans to ſulphurated hidrogen gas, on account of its poſſeſſing the properties of an acid.

If

Explodes with oxigen.

If two parts of arſeniated hidrogen be mingled with three of oxigen, and a taper be preſented to the mixture, an exploſion takes place; no metallic arſenic is ſeparated, but the products are arſenious acid and water: ſoap-bubbles with the mixture of theſe gaſes, explode with a bluiſh white flame, leaving a white ſmoke and ſtrong alliaceous odour. Equal parts of arſeniated hidrogen and oxigen gaſes, fired in like manner, do not explode ſo loudly, but the report is accompanied with a much more vivid flame. A ſtream of arſeniated hidrogen, iſſuing from a bladder fitted with a ſtop-cock, and ſet to burn in a large receiver filled with oxigen, yielded arſenic acid. The combuſtion in this manner is uncommonly beautiful; the gas burns with a blue flame of uncommon ſplendor.

Burns beautifully in oxigen gas.

Two parts of arſeniated hidrogen, and one of oxigen gas, being detonated in a cloſe veſſel by means of the electric ſpark, left a ſmall reſiduum; on repeating the experiment, the detonating tube broke during the exploſion, which prevented the examination of the reſidue. From what has been ſo far related, it becomes evident that the conſtituent parts of arſeniated hidrogen gas, are metallic arſenic and hidrogen. Were it poſſible to determine with abſolute certainty, that no increaſe of volume took place during the ſolution of arſenic in hidrogen, the proportion of the conſtituent parts of this gas might be aſcertained thus:

Compoſed of metallic arſenic and hidrogen.

	French weight and meaſure.
One cubic inch of hidrogen, weighs -	0,0353
One cubic inch of arſeniated hidrogen, weighs	0,2435
Deducting the former from the latter, we get	0,2082

Which is the quantity of arſenic diſſolved in the gas, conſequently one cubic inch of arſeniated hidrogen gas, conſiſts of 0,0363 hidrogen, and 0,2082 arſenic; and one cubic inch of this gas contains about $\frac{1}{4}$ grain of metallic arſenic.

Habitudes of arſeniated Hidrogen to Acids.

Its habitudes to acids. Concentrated nitric acid.

Into a phial, containing about eight cubic inches of arſeniated hidrogen, I poured a half cubic inch of concentrated nitric acid. The moment the acid came into contact with the gas

gas. The phial was filled with denſe red fumes, a white flame pervaded the veſſel, and a detonation enſued.

On repeating the experiment with dilute nitric acid, no accenſion took place. The reſiduary gas was pure hidrogen, and the water contained arſenic acid. Fuming concentrated nitrous acid therefore is capable of oxidiſing the arſenic contained in this gas, at the ſame time that the oxigen of the acid burns with the hidrogen of the gas, and produces water; whereas weak nitric acid is only capable of oxidizing the arſenic, without acting upon the hidrogen preſent. Dilure nitric acid.

Into a glaſs tube, furniſhed with a ſtopper at one extremity, and cloſed at the other, I introduced eight cubic inches of arſeniated hidrogen, to which were added two cubic inches of nitro-muriatic acid. After having agitated the fluids, on opening the tube under water, a diminution of one cubic inch took place. The reſiduary gas was pure hidrogen. It is remarkable that, during the addition of the nitro-muriatic acid, a black powder ſeparated, which again diſappeared on agitating the tube. Nitro-muriatic acid acts therefore in the ſame manner upon this gas, as oxigenized muriatic acid gas. It effects firſt a ſeparation of the metallic arſenic, and then oxiginizes this metal. Liquid oxigenized muriatic acid decompoſes arſeniated hidrogen by mere agitation; the reſidue is hidrogen. Muriatic acid exerciſes very little action upon arſeniated hidrogen; but merely diſſolves a minute portion of it, which may be expelled again by heat. Concentrated acetic acid has no effect upon it. Nitro-muriatic acid. Oxiginized muriatic acid. Muriatic acid. Concentrated acetic acid.

Into a glaſs cylinder holding eight cubic inches of arſeniated hidrogen, I poured one cubic inch of concentrated ſulphuric acid, and then cloſed the tube. At the moment of the addition of the acid, the cylinder became lined with a coat of bright metallic arſenic, ſo as to reſemble a looking-glaſs. On agitating the cylinder, the coating reſolved itſelf into a browniſh black powder, which, after a few days, aſſumed the colour of Kermes mineral. On opening the cylinder under water, a diminution of bulk enſued, and the reſiduary gas proved to be hidrogen. The experiment was repeated, and yielded the ſame reſults. The ſulphuric acid employed in this experiment, had acquired a penetrating pungent ſmell, and was examined, after having been neutralized by ammonia, in the following manner: Concentrated ſulphuric acid. Curious phenomenon. The ſulphuric acid examined.

Ammoniate of copper, on being mingled with it, acquired a greeniſh colour. Hydroſulphuret of ammonia inſtantly occaſioned a copious yellow precipitate.

Water impregnated with ſulphurated hidrogen gas, occaſioned a ſimilar effect. From the reſults of theſe teſts it becomes obvious, that the acid conſiſted of ſulphuric, ſulphureous and arſenic acid. In order to be certain in this reſpect, I mingled a few drops of liquid arſenic acid with a mixture of ſulphuric and ſulphureous acid, neutralized the fluid with ammonia, and ſubmitted it to the ſame teſts. The reſults of this mixture were analogous to the former. The decompoſition of the arſeniated hidrogen gas, is probably analagous to the decompoſition of this gas, by means of oxiginized muriatic acid gas. The ſulphuric acid firſt gives up part of its oxigen to the hidrogen of the arſeniated gas, and occaſions the ſeparation of the arſenic; which, at the expence of the remaining portion of oxigen of the ſulphuric acid, becomes afterwards oxigenized, and conſtitutes the arſenic acid.

Theory.

Habitudes of arſeniated Hydrogen Gas to Metallic Solutions.

Its habitudes to metallic ſolutions.

I cauſed a current of arſeniated hidrogen gas, to paſs through a ſolution of ammoniate of copper. A metallic pellicle appeared on the ſurface of the fluid, which ſuffered no other change.

Ammoniate of copper.

Into a bottle filled with arſeniated hidrogen gas, I dropped a ſolution of muriate of tin. On agitating the ſolution, it acquired a brown colour, a partial diminution of the gas enſued, but the ſolution of tin was not converted into an oxidized muriate of tin, which would have been the caſe, if the arſenic exiſted in the gas in an oxidized ſtate.

Muriate of tin.

Nitrate of lead, on being brought into contact with arſeniated hidrogen gas, became turbid, and depoſited a precipitate, which was arſeniate of lead.

Nitrate of lead.

Nitrate of ſilver ſubmitted to the action of the gas, became inſtantly of an intenſe black, and a pellicle of metallic ſilver collected on the ſurface of the fluid. The reſidue of the gas, which had been made to act on the oxide of ſilver for ſome time, had all the properties of pure hidrogen.

Nitrate of ſilver.

This experiment ſhows, that nitrate of ſilver might be employed for detecting the preſence of arſeniated hidrogen; for

A good teſt.

as

as long as a minute quantity of arſenic was preſent, a black precipitate enſued, whereas *pure* hidrogen has no effect upon this re-agent.

I paſſed into a concentrated ſolution of nitrate of ſilver, a ſtream of arſeniated hidrogen, collected the black metallic precipitate, waſhed and dried it. The fluid obtained in this proceſs did not diſturb the tranſparency, or change the colour of ammoniate of copper. Neither liquid ſulphurated hidrogen, tincture of galls, nor potaſh, had any effect upon it. It contained therefore neither ſilver nor arſenic. The precipitate before obtained, acquired a metallic luſtre on being ſaturated; laid on ignited coals, it diffuſed an odour of arſenic, and it yielded by fuſion a button of ſilver. It was an arſeniate of ſilver.

Nitro-muriate of gold.

Arſeniated hidrogen paſſed into a ſolution of nitro-muriate of gold, occaſioned a precipitate; on the ſurface of the fluid appeared a pellicle of metallic gold; and the ſides of the veſſel, in contact with the fluid, became beautifully gilded. The fluid through which the gas had been paſſed, examined in the uſual manner, proved to contain no veſtige either of gold or arſenic. The precipitate greatly reſembled charcoal duſt, interſperſed with minute particles of gold.

Decompoſes metallic ſolutions.

It is highly probable, that arſeniated hidrogen is capable of decompoſing all metallic ſolutions, the baſis of which is either nitric, or muriatic acid, and probably other acids.

Habitudes of arſeniated Hidrogen Gas to various other Bodies.

Its habitudes to expreſſed oils,

Expreſſed oils, on being agitated for ſome time in contact with arſeniated hidrogen, abſorbed part of the gas, and acquired a deeper colour.

and to alcohol, and alcalies.

Alcohol ſuffers no change from arſeniated hidrogen. Solution of potaſh, and liquid ammonia, do not abſorb it.

Such are the properties of this gas, the inveſtigation of which I ſhall continue as ſoon as my health is reſtored, it being ſo conſiderably injured by the unavoidable inhalation of this gas during the courſe of theſe experiments, which gives me ample reaſon to conclude, that the gas muſt be highly poiſonous.

D. J. B. TROMSDORFF.

Enfurth, Feb. 1803.

VIII.

Account of an Eudiometric Apparatus, contrived and uſed by Dr. HOPE, *Profeſſor of Chemiſtry in the Univerſity of Edinburgh*.*

Dr. Hope's eudiometric apparatus.

SINCE the diſcovery of the uncertainty with which the application of nitrous gas to atmoſpheric air, and other mixtures, containing oxigen is attended, it has been found deſirable to preſent ſolid or liquid ſubſtances for the abſorption of that principle. This, on firſt conſideration, may ſeem at leaſt as eaſy to be done, as to mix two gaſes; but it is by no means ſo, becauſe the liquids in particular poſſeſs a degree of chemical activity, which renders it inconvenient to immerſe the hands in them, or to expoſe their ſurfaces to the open air, eſpecially when it is attempted to accelerate their operation by means of agitation, ſo as to obviate the principal objection to their uſe, the tardineſs of the proceſs.

The apparatus of Dr. Hope, which he uſes in his lectures and in his experiments, is at once ſimple aud effectual, and I have the pleaſure of inſerting the following correct deſcription with his permiſſion.

The apparatus conſiſts of two bottles, which are repreſented in *Plate* XII, connected together in the manner in which they are uſed; A repreſents a ſmall bottle which may be nearly two inches in external diameter, and three in length, having a neck and ſtopper at D, and another neck as uſual at C. It is deſtined to contain the eudiometric liquor. B repreſents a larger bottle, which may be nearly of the ſame diameter, or rather of ſomewhat leſs, but $8\frac{1}{2}$ inches long. The neck of B is fitted accurately by grinding into the neck of A at C.

The method of uſing this apparatus is very ſimple: introduce in the ordinary way into the bottle B, the air or gas

* As the deſcription at p. 61, of the preſent volume is in ſome reſpects inaccurate through haſte, and the figure, being an outline, appears as if the neck of the upper veſſel protruded ſo far into the lower, as to prevent the aſcent of a portion of the gas after agitation,—I have choſen rather to give an entire deſcription and drawing in this place, than adopt the leſs acceptable proceſs of annotating and correcting.

till

till it is full; then fill A with the abſorbing liquor, for example with a ſolution of ſulphuret of lime, which Dr. Hope commonly employs, and covering the mouth with a flat piece of glaſs, plunge it under the ſurface of water, and there inſert the neck of B. Dr. Hope's eudiometric apparatus.

The compound veſſel is then removed from the water, and inclined till a ſufficient quantity of the liquor flows into B. It is now well ſhaken, and the agitation ought to be continued till the abſorption is compleated—Leſt the diminution of the denſity of the included elaſtic fluid ſhould retard the abſorption of the oxigenous portion; from time to time the apparatus, in the poſition in which it is repreſented in the figure, is to be placed in a plate full of water, and the ſtopper D is to be looſened, or ſo far withdrawn, as to allow this fluid to enter to fill the place of the abſorbed gas.—By this admixture of water the liquor is diluted, but not to ſuch a degree as in any meaſure to interrupt the advancement of the proceſs, unleſs indeed when the gas abounds very much in oxigen.

When a gas of this deſcription is the ſubject of experiment, it may be proper to uſe an apparatus, of which the bottle A is made of greater capacity in relation to the ſize of B, than in the proportion already aſſigned.

As ſoon as it is obſerved, that after reiterated agitation, and opening the ſtopper D, the liquor does not riſe higher, the abſorption may be conſidered as compleated, and the operation may be finiſhed by allowing the inſtrument to regain its original temperature, in caſe, from want of due precaution, it may have been affected in this reſpect by the warmth of the hand in the courſe of the experiments.

If the bottle B be graduated, the amount of the abſorption may be determined at once, by plunging the apparatus into water to the level of the included liquid, and removing the ſtopper, otherwiſe the reſidual gas may be transferred into a tube, expreſsly graduated for meaſuring gaſes.

By this convenient contrivance, we ſee that the liquid is economized and the celerity, neatneſs, and preciſion of experiment are enſured. The ſize here mentioned is very well adapted to the purpoſes of public exhibition, but it is almoſt needleſs to remark, that it may, and in general ought to be made conſiderably ſmaller for the ordinary eudiometric experiments.

Dr. Hope ſuggeſted that the apparatus might be made ſtill more ſimple without impairing its merits, in any conſiderable degree, particularly when ſmall volumes of gas are to be examined. This is done by uſing a ſmall bottle having one neck only, and having a graduated tube nine or ten inches long, and from half to three quarters of an inch in diameter, accurately adjuſted to fit into it, but not projecting into its cavity.

If the bottle have twice or thrice the capacity of the tube, the ſame ſolution of ſulphuret of lime may be repeatedly uſed, and the abſorption will be more expeditious.

In employing this inſtrument, the manipulation is in all reſpects the ſame as above deſcribed, excepting when the progreſs or termination of the operation is to be diſcovered. For this purpoſe, looſen the connection between the tube and the bottle, in a degree ſufficient to allow the ingreſs of the water of the trough, in which the eudiometer muſt then be immerſed.

This apparatus equally unites diſpatch, œconomy of eudiometric liquor, and convenience of management.

IX.

Deſcription of an Apparatus for drying the Products of Chemical Analyſis which is alſo uſeful for Experiments of Congelation. By Mr. Frederick Accum. *Communicated by the Inventor.*

Apparatus for drying precipitates on the water bath:

THIS apparatus, Plate X. is extremely uſeful in drying ſuch products as abſolutely require a temperature not exceeding 212°; ſuch as fulminating mercury, Chenevix's fulminating ſilver, and other exploſive compounds. The ſubſtance to be dried muſt be placed in the conical glaſs veſſel B, and when the veſſel F is filled with water up to the ſide tube D; the deſiccation may be performed without any riſk of exploſion, or any further trouble, by putting the apparatus over a lamp, and keeping the water in a ſtate of ebullition. *(See the lower drawing in perſpective.)* I have found it particularly uſeful in the deſiccation of the precipitates obtained in the analyſis of minerals. It is well known that the ſame mineral, analyſed

analyſed by different chemiſts, has been found to yield different proportions of the ſame ingrediants, and that the difference of proportions of the conſtituent parts, in many caſes, is often more apparent, than real; ariſing entirely from the various degrees of deſiccation that has been employed by different analyſts, and ſometimes even by the ſame perſon. This point is of ſuch importance, and is productive of ſo much trouble, that every chemiſt who has analyzed a mineral water, or cryſtallized and ſeparated ſmall quantities of deliqueſcent ſalts, will at once perceive the utility of the apparatus in this reſpect.

The ſame uſed as a water bath:

This apparatus may likewiſe be uſed as a *water-bath*. In that caſe, the conical glaſs veſſel B is removed, and the inner tin veſſel E filled with water; into this, retorts, flaſks, gallipots, vials, bottles, &c. may be immerſed for promoting the proceſſes of diſtillation, digeſtion, ſolution, evaporation, &c. or it may be uſed as a *ſand bath*, (it being hard ſoldered) by filling the tin veſſel with ſifted ſand, for performing thoſe operations which require a higher temperature, than that of boiling water.

or ſand bath.

Method of freezing (mercury for example) by this inſtrument.

When the inſtrument is required to be uſed as a *freezing apparatus*, the bottom cover G is to be taken off, and the cavity between the interior, and exterior veſſel, filled with the frigorific mixture; a wetted piece of bladder is then to be tied over the opening, or the cover is put on, to retain the mixture. The ſecond frigorific material (for inſtance if quickſilver is to be frozen) conſiſting we will ſay, of muriate of lime and ſnow, are to be cooled by the mixture in the exterior veſſel, by putting the muriate of lime into the conical glaſs veſſel, together with the mercury contained in a thin glaſs tube; and ſurrounding the glaſs veſſel, by filling the interior tin veſſel with ſnow, or pulverized ice. When theſe materials have been cooled down to 0°; the ſnow and ice may be mixed together by emptying the muriate of lime into the veſſel containing the ſnow, and ſtirring the mixture with a glaſs rod to facilitate the ſolution of the ſalt, and to produce the requiſite degree of cold. The number of apparatus I have ſold to philoſophical chemiſts, gives me reaſon to ſuppoſe, that they have proved uſeful.

FREDERICK ACCUM.

11, *Old Compton Street,*
Soho.

Letter

X.

Letter from Mr. ACCUM, *in anſwer to the Enquiries of a Correſpondent reſpecting the Proceſs for obtaining the Aguſtine Earth.*

To Mr. NICHOLSON,

DEAR SIR,

Miſtake reſpecting the publication of the method of obtaining agustine earth.

YOUR correſpondent, P. O. in the laſt number of your Journal, is correct, when he obſerves, that the proceſs for obtaining aguſtine earth is not noticed in my ſyſtem of practical chemiſtry, nor in any other work publiſhed in this country, and alſo that moſt of the books he quotes, were publiſhed a conſiderable time after this earth *was made known* by profeſſor Tromsdorf; but he is miſtaken in his opinion, that it was known in this country previous to the publication of the above works.

Firſt account.

The method of ſeparating this earth from the mineral which contains it, had not then been communicated to us through the uſual channels of ſcientific information. The firſt account of the method of ſeparating this earth I can find, is contained in a german work, entitled *Practiſche Anleitung zur zerlegenden Chiemie*, publiſhed by Profeſſor Göetling, 1802. From which the following tranſlation is made.

The proceſs. Saxon beril pounded; boiled with potaſh; then fuſed;

Let a determinate quantity of the Saxon beril finely levigated, be boiled in a ſilver veſſel, with three or four times its weight of potaſh, diſſolved in a ſufficient quantity of water; evaporate the whole to dryneſs, and fuſe the maſs.

ſoftened by water; diſſolved in muriatic acid; evaporated to dryneſs; diluted with water; and the ſilex ſeparated;

Soften the alcaline maſs by the gradual addition of water, and when detached from the crucible, add to it muriatic acid till the whole is diſſolved. Evaporate the ſolution to dryneſs, boil the maſs in a ſufficient quantity of water, and ſeparate the inſoluble reſidue [ſilex] by the filtre.

the fluid precipitated by carbonate of ſoda;

Decompoſe the fluid from which the ſiliceous earth has been ſeparated, by gradually mingling it with a ſolution of carbonate of ſoda; collect the precipitate, and waſh it repeatedly.

ſeparate the alumine from the precipitate by potaſh which diſſolves it and leaves the *aguſtine earth*.

When the precipitate obtained in the laſt proceſs has acquired ſome conſiſtence, tranſfer it into a flaſk containing a concentrated ſolution of potaſh. The alumine which was preſent

preſent in the mineral, will be diſſolved*, and the inſoluble reſidue left, is the new earth called *Aguſtine*.

It is diſtinguiſhed from all other earths by being abſolutely inſoluble in potaſh, ſoda, and ammonia, and all their carbonates. Nor can an union of either of the two firſt alcalies with Aguſtine be effected by fuſion. It is ſoluble in acids, with which it forms ſalts, which have little or no taſte. It is ſoluble in acids with equal facility after having been ignited, as when freſh prepared. It fuſes with borax into a tranſparent colourleſs glaſs. Characters of this earth.

100 parts of the Saxon beril yielded Profeſſor Tromſdorf 78,0 Aguſtine, 4,5 alumine, and 15,0 ſilex.

I am, Sir,

Your moſt obedient,

FREDERICK ACCUM.

11, *Old Compton Street, Soho,*
15th October, 1803.

XI.

Letter from a Correſpondent concerning the Method propoſed by Mr. Carliſle for cloſing wide-mouthed Veſſels.

October 15, 1803.

SIR,

IN the laſt number of your Journal, publiſhed on the firſt of this month, page 68, I find a letter addreſſed to you by Mr. Carliſle, deſcribing "a method of cloſing wide mouthed veſſels intended to be kept from communicating with the air;" and in *Plate* V. *Fig.* 2, there is an engraving of the veſſel re- The method of cloſing veſſels ſuggeſted in our laſt number

* It is perhaps needleſs to ſtate that the alumine may be ſeparated from the alcaline ſolution, by ſaturating it with muriatic acid in exceſs, ſo as to neutralize not only all the potaſh, but alſo to diſſolve the alumine, and then to decompoſe the obtained ſolution by carbonate of ammonia. Should glucine be expected, the carbonate of ammonia ſhould be added in conſiderable exceſs; for the exceſs of ammonia retains in ſolution the glucine, and nothing but alumine will be thrown down. The glucine may be obtained by evaporating the ſolution to dryneſs and igniting the reſidue.

F. A.

com-

commended by the above ingenious gentleman. But I muſt beg leave to obſerve that this, which in your Journal is denominated "a new method, by a jar, the cover of which fits into a groove with hog's lard," is in reality a very old method; for in the quarto edition of Buffon, publiſhed at Paris in 1749, Tome III. p. 192, you will find many ſuch veſſels repreſented, and *Fig.* 4, is preciſely the ſame as the one given in your work. But Buffon was not the inventor of this method, for it is claimed by the celebrated Mr. Le Cat, who had glaſſes made upon this conſtruction about the year 1739, and in 1748 ſent to the Royal Society a deſcription, which, with an engraving, may be ſeen in the 46th volume of the Philoſophical Tranſactions, page 6.

Was uſed by Buffon;

and by Le Cat;

But even the celebrated Mr. Le Cat was not the inventor, for old Glauber employed this method long before, and a plate and deſcription may be ſeen by any one who will take the trouble to conſult his "*Fornacum Philoſophicarum, pars quinta,*" page 13, &c. &c. publiſhed at Amſterdam in 1661. The only difference is, that Glauber uſed quickſilver to fill the groove; Mr. Le Cat employed quickſilver or oil; and Mr. Carliſle recommends hog's lard. If you think theſe remarks worthy of a place in the next number of your Journal, they are much at your ſervice; from an

nd before all by Glauber.

OLD CORRESPONDENT.

XII.

Account of an Experiment for ſupplying Worm Tubs and other Refrigeratories by the aſſiſtant Preſſure of the Atmoſphere, which proved unſucceſsful, on a large Scale; to which is added an Improvement for extending the uſeful Applications of the Syphon. By EDWARD HOWARD, ESQ. F. R. S. *In a Letter to the Editor.*

To Mr. NICHOLSON.

SIR,

Apparatus for more eaſily raiſing water in worm tubs, by atmoſpheric preſſure.

THE method of ſupplying worm tubs and condenſers, given by Sir Alexander Edelcrantz in your laſt number, induces me to trouble you with the reſult of an unſucceſsful experiment, which I ſome time ſince made on much the ſame principle, and

and on a worm tub of a conſiderable ſize. Indeed it was my firſt intention to have made the worm tub part of the arm of a ſyphon; but, as warm water was conſtantly wanted in an elevated part of the premiſes, and as all the water uſed was raiſed by a lifting pump from an adjacent well, an opportunity appeared to preſent itſelf, both of economizing labour, and of making uſe of the warm water of the upper ſurface of the worm tub. To effect theſe objects, I made the pipe coming from the well immediately communicate with the tub, and annexed the pump, by means of another pipe, to the upper ſurface of the tub. By this conſtruction, there was every reaſon to conclude that it would be practicable to pump off the warm water, and alſo, that whenever the pump was worked to ſupply the other demands of the laboratory, the water in the worm tub would be conſtantly changed without additional labour. It was further thought neceſſary to add a valve of ſafety to the upper part of the tub, and two ſtop cocks, one on the pipe leading from the well to the tub, and the other on the pipe leading from the pump to the tub, in order that by the regulation of theſe cocks, water might be had either immediately from the well, for other purpoſes of condenſation, &c. or it might be drawn from the worm tub for proceſſes requiring warm water, or for uſes to which warm or cold water might be indifferently applied.

To give a better idea of the apparatus, I have ſubjoined the following outline, *Plate* XII. where A repreſents the worm tub; B a pipe leading from the well; C a pipe communicating immediately from the pump; D a valve of ſafety, E a pump; F a well; and G G ſtop cocks.

The apparatus when made upon a ſmall ſcale, with a Woulfe's bottle and glaſs tubes, anſwered perfectly and promiſed to be a valuable acquiſition. I wiſh I could relate the ſucceſs of the ſame experiment made upon a large one: But, notwithſtanding the beſt workmen in London were employed, they could not make the joints of the worm tub ſufficiently tight to reſiſt the preſſure of the atmoſphere for more than a few ſucceſſive hours.

The apparatus ſucceeded perfectly in the ſmall way; but failed on a large ſcale.

I ſhould not, Sir, have offered to you the reſult of an unſucceſsful experiment were it not from an apprehenſion that the worm tub recommended by Sir Alexander Edelcrantz might be no leſs difficult to conſtruct than the one I have deſcribed.

Allow

Improvement in the ſyphon.

Allow me, Sir, to take advantage of this opportunity to communicate what I believe to be an improvement of the ſyphon. It was made at the time I had an idea of applying the principles of a ſyphon to the worm tub: Although it may not be applicable to this purpoſe, it will be found exceedingly uſeful in caſes where it would be diſagreeable, dangerous, or impoſſible, to exhauſt the ſyphon by the common mode of ſucking out the air, if I may be allowed ſuch an expreſſion. I think it even probable that ſyphons of conſiderable dimenſions may be introduced for emptying ponds, or for lowering the water in mill-dams or canals; for it is without doubt deſirable to avoid cutting through an embankment, and a very large inſtrument of the new conſtruction would be thus put in action with great eaſe. The improvement, Sir, I have thus ventured to ſpeak of conſiſts merely in enlarging the exhauſting pipe to the ſame calibre as the reſt of the ſyphon; in elevating it a little, and in opening its mouth like a funnel. See *Fig.* 3. *Plate* XI.

It is ſcarcely neceſſary to point out that, to uſe ſuch a ſyphon, the ſhort arm is, as uſual, to be immerſed in the liquor intended to be operated upon, and the aperture of the long arm to be cloſed whilſt the whole inſtrument is to be filled through the funnel with ſome of the ſame liquor.

I am,

SIR,

Your obedient humble ſervant,

EDWARD HOWARD.

XIII.

A Method of equalizing the Motion of a Steam Engine without the Aſſiſtance of a Fly Wheel. By Mr. ARTHUR WOOLF, *Engineer. Communicated by the Inventor.*

Equalizing mechaniſm which allows the engine to ſet off or ſtop in any part of the ſtroke.

THE mechaniſm here preſented as a ſubſtitute for the fly, poſſeſſes the advantage of equalizing the motion, with the power of being ſtopped and ſet to work at any part of the ſtroke, the utility of which, in mines, collieries, and other works, will be immediately ſeen by thoſe converſant in ſuch undertakings.

Plate

Plate XI. *Fig.* 1. A repreſents part of the engine beam; B the connecting rod; C the crank arm; D a cog-wheel, working into another cog-wheel F, of half the ſize; F a crank arm on the ſhaft of the ſmall wheel; G a cylinder cloſed at bottom, in which a ſolid or unperforated piſton moves, leaving a vacuum beneath. This acts ſimply inſtead as a weight on the crank F, by the conſtant preſſure of the atmoſphere; and the diameter of the piſton muſt be ſuch as nearly to equal one third of the power of the engine.

In *Fig.* 2. the outer circle is the line deſcribed by the crank; the circumference of the inner circle is equal to twice the diameter of the outer, and the ſquare has the ſame circumference; this laſt exhibits the inequality ſtill remaining, which by this method is reduced to about one fifth; but by the aſſiſtance of a ſmall fly on the ſecond motion, the effect will become nearly the ſame as that of a rotative engine, with the advantages here mentioned.

The ſame motion may be applied to a pump, but in this caſe the two cranks muſt be horizontal at the ſame time.

XIV.

Improvement by which the additional Arc in Mr. Ezekiel Walker's reflecting Quadrant is rendered unneceſſary. In a Letter from the Inventor.

To Mr. NICHOLSON.

SIR,

ANY one who has a juſt idea of the reflecting quadrant, deſcribed on page 218 of the fourth volume of your Journal, will perceive that one half of the arc of that inſtrument is appropriated ſolely to the rectifying of the ſecond horizon glaſs. This method of adjuſting is as good as any other that I have to propoſe, but it is attended with the inconvenience of adding to the ſize of the inſtrument, and conſequently to its weight. Improvements in Mr. E. Walker's reflecting quadrant.

The following method of adjuſting the ſecond horizon glaſs will reduce the inſtrument to an octant, which will ſtill poſſeſs the ſame property of meaſuring any angle leſs than 120° by the fore obſervation.

Firſt

Improvement in Mr. E. Walker's reflecting quadrant.

Firſt, let a ſmall mirror be fixed upon the top of the index glaſs. This rectifier muſt be placed parallel to the firſt horizon glaſs, when the index ſtands at 90° on the arc, conſequently the index glaſs and rectifier will form an angle of 45 degrees. Secondly, let the two horizon glaſſes ſtand as high above the plane of the octant as the rectifier, and the inſtrument is ready for uſe.

After the firſt horizon glaſs has been adjuſted, bring the index to 90, and the rectifier will be parallel to the horizon glaſs, if the index glaſs and rectifier form an exact angle of 90° on the arc; but if they do not form that angle, the index will ſhow the error. Then to determine the error of the ſecond horizon glaſs, let the index be brought to 0, and the rectifier will in that ſituation perform the ſame office as the index glaſs in the quadrant, when the index ſtands at 90 at N; but as this has been fully explained in the deſcription of that inſtrument, it need not be further inſiſted on here.

An octant of five inches radius, conſtructed on theſe principles, would be exceedingly portable, and ſo ſtrong as not to be eaſily deranged by carriage; and theſe are properties which may recommend it to the attention of the traveller by land, particularly if he viſit thoſe latitudes, where the altitude of the ſun ſometimes exceeds 60 degrees.

Theſe two glaſſes may alſo be added to the ſextant, without depriving it of any of the valuable properties which it now poſſeſſes: and this additional apparatus need only be uſed in taking ſuch angular diſtances as are beyond the power of that inſtrument.

I am,
SIR,
Your's reſpectfully,
E. WALKER.

Lynn Regis, October 15th. 1803.

Plate XI. *Fig.* 3. A B repreſents the index glaſs; C D the rectifier; *m* the horizon glaſs; *n* the ſecond horizon glaſs.

SCIENTIFIC NEWS.

Abſtract of Cit. SEGUIN's *Inquiries concerning Fermentation* *.

IN his firſt paper Cit. Seguin explains the plan of inquiry he has undertaken concerning fermentation in general; and more particularly concerning the making of beer, wine, cider, malt and melaſſes ſpirits, &c. In his ſecond his object is to prove, that fermentation is not produced by a ſubſtance *ſui generis*, but by a combination of circumſtances.

Fermentation not produced by a peculiar ſubſtance, but by a combination of circumſtances.

He ſhows, that, in the caſe of clear liquors fermenting, the true ſolvent of the fermenteſcible cauſe, whatever it be, is water, and not the ſaccharine matter; that the continuance of contact, and the preſence of ſugar, are by no means neceſſary for the ſolution of any fermenteſcible principle in the yeaſt; that this ſolution is made by water in a very ſmall quantity, it is true, but almoſt ſuddenly, and even in the ordinary temperature of the air; finally, ſuppoſing ſugar alſo to poſſeſs the property of diſſolving any given fermenteſcible principle, it would be impoſſible to demonſtrate this, ſince, to render it perceptible, the ſugar muſt previouſly be diſſolved in water.

Water the true ſolvent of the fermentative cauſe, and this ſpeedily, and without the preſence of ſugar being neceſſary.

Additional Experiments of Mr. RITTER, *of Jena, on Galvanic Phenomena* †.

NATURAL philoſophers will learn with pleaſure, that Mr. Ritter continues his elegant experiments on a ſubject, to the progreſs of which he has ſo greatly contributed. As they all relate to a known theory, we ſhall content ourſelves with giving the reſults as communicated to us by Mr. Orſted, leaving to the experamentaliſt the taſk of proving them with all the requiſite minuteneſs.

The object of Mr. Ritter being to compare the electricity of Voltas' pile with that of electrical machines, he conſiders ſucceſſively the intenſity of electricity, chemical action, ſpark, and ſhock, in the pile.

Mr. R's object to compare Volta's pile with electrical machines.

* Bulletin des Sciences, No. 75.

† Bulletin des Sciences, No. 77.

As

Electricity positive at one pole of the pile, negative at the other, diminiſhes between them, null in the centre.

As to the intenſity, we know that the electricity is poſitive at one of the poles of the pile, and negative at the other; it has been ſhown likewiſe, that it diminiſhes between theſe two extremes, ſo as to be null in the middle of the pile. Mr. Ritter ſought to compare the degrees of intenſity at theſe two poles, and thoſe of different piles. This he attempted by determining the time neceſſary for charging a given battery; but this method is inaccurate, and no certainty can be attained in this reſpect, but by means of the electrical balance.

The time neceſſary for charging a battery, an inaccurate meaſure of intenſity.

According to Mr. Ritter, the action of the poſitive pole of the pile diſpoſes metals to combine with oxigen, and that of the negative pole diſpoſes them to combine with hydrogen. If the poſitive pole be armed with a gold leaf, and the negative with a bit of charcoal, on forming a communication between theſe two ſubſtances the leaf of gold burns with a brilliant light, and the charcoal remains untouched: but if the charcoal be placed on the poſitive ſide, and the gold on the negative, the charcoal burns, and the gold is melted. If the negative pole be brought into contact with the ſhining ſurface of quickſilver, it leaves a trace different from that produced by the poſitive pole.

+ Electricity of the pile diſpoſes metals to combine with oxigen,
— with hidrogen.
+ pole armed with gold leaf,— with charcoal, on forming a communication the gold leaf is burned;
if the charcoal be on the + ſide, it burns, and the gold is melted.
— pole, brought into contact with quickſilver leaves a mark on its ſurface different from that of the +.

Mr. Ritter aſſerts, that all the effects of the pile on the animal body are reducible to expanſions and contractions. All the parts of the human body aſſume an increaſed bulk on the contact of the poſitive pole, and contract on the contact of the negative: for inſtance, the action of the poſitive pole on the tongue produces there, at the expiration of a few minutes, a ſlight elevation, whereas the negative pole occaſions a little depreſſion. If the ſame perſon touch the two poles with the two hands wetted, the intenſity of the pulſe is increaſed in the hand in contact with the poſitive pole, while its ſtrength is diminiſhed in the other, but the number of pulſations continues the ſame in each. The expanſion thus produced in the organs, is attended with a ſenſation of heat, the contraction with a ſenſe of cold.

Theeffectsof the pile on the animal body all reducible to expanſions and contractions.
The + pole increaſing the bulk of parts, the — diminiſhing it.
Effects on the tongue, and on the pulſe.
The expanſion occaſions a ſenſe of heat, and *vice verſa*.

If the eye be made to communicate with the poſitive pole, it ſees objects red, larger, and more diſtinct; in contact with the negative pole it ſees them blue, ſmaller, and more confuſed. The tongue receives from the poſitive pole an acid taſte, from the negative an alcaline. The ear being in con-

To the eye the

tact

tact with the former, all ſounds ſeem more grave: with the latter, more acute.

+ pole makes objects red, large & diſtinct; the —, blue, ſmall, and confuſed.

In general the two poles of the pile produce oppoſite effects.

Such are the reſults of Mr. Ritter's experiments. We have no opportunity of verifying their accuracy; but their ſingularity, their number, and particularly the ingenuity of their author, lead us to preſume, that this account of them will be read with pleaſure.

+ gives an acid taſte,— alcaline.
+ deepens ſounds.
— lightens them.
The two poles generally produce oppoſite effects.

SOCIETE PHILOMATH.

Abſtract of ſome Remarks on the Acetite of Lead, by Cit. THENARD *.

A MANUFACTURER of acetite of lead was obliged to ſtop his works, being no longer able to make the ſalt cryſtallize in needles, but always obtaining it in laminæ, which induced purchaſers to refuſe it; Cit. Thenard inquired into the cauſe of this phenomenon, and ſoon perceived, that it was owing to the proportions of the conſtituent principles of the ſalt. He ſucceeded in forming a ſalt perfectly ſimilar to it, by boiling in water a hundred parts of the acetite of lead of the ſhops, with a hundred and fifty parts of litharge well dried, and deprived of carbonic acid by means of fire. Analyſis corroborated the exiſtence of two ſpecies of acetite of lead; one, long known, conſiſts of oxide of lead 0.58, acetous acid 0.26, and water 0.16, the other, which has hitherto eſcaped the notice of chemiſts, contains oxide of lead 0.78, acetous acid, 0.17, water 0.05.

A manufacturer obliged to ſtop his work, becauſe his acetite of lead was always in the forms of laminæ.

Two ſpecies of acetite of lead.

The former of theſe ſalts has an exceſs of acid, and a ſtrongly ſaccharine taſte; cryſtallizes in needly priſms, which appear to be hexagonal, and terminated by hexaedral pyramids; undergoes no alteration in the air; is very ſoluble in water, and forms with it a ſolution feebly precipitable by carbonic acid. The latter, on the contrary, is neutral; has a leſs perceptible ſaccharine taſte; affects a lamellated figure; is ſoluble in vinegar, and then exhibits on evaporation the needly form;

The common or that with exceſs of acid.

The new or neutral acetite.

* Bulletin des Sciences, No. 77.

efflorefces

efflorefces flightly in the air; is much lefs foluble in water, but forms with it a folution abundantly precipitable by carbonic acid. This precipitate is very white, forms a paſte with oil, and by extracting the carbonic acid from chalk by means of fire, it would perhaps be poſſible to prepare a fine white-lead by theſe means.

Fine white lead may be prepared from it.

The value of the diſcovery of this ſalt, will be readily perceptible. It not only brings us acquainted with a new ſubſtance intereſting in a ſcientific view, as it affords us a freſh proof, that the proportions of the conſtituent principles of ſalts may vary greatly; but it is likewiſe of importance in the art of phyſic, in which ſalts of lead are daily employed, and which may require one containing a large proportion of oxide; and it is of conſequence to the arts in general, as it affords a new method of obtaining a fine white-lead, and particularly to that of manufacturing acetite of lead, on the proceſſes and products of which it throws great light.

Advantages of the diſcovery.

The Arachis Hypogæa, or Ground Nut of the Weſt Indies, cultivated in France for its Oil.

IN the departments of Landes and l'Herault in France, lat. $43\frac{1}{2}^{\circ}$ to $44\frac{1}{2}^{\circ}$, an oily plant, called arachis, of the family of lentils, (arachis hypogæa) begins to be cultivated. It was brought by the Spaniards from Mexico, and was introduced by the French from Spain. An ounce of the oil of this plant, with a wick a line and half in diameter, burned nine hours and twenty-ſix minutes; an ounce of olive oil under ſimilar circumſtances, laſted only eight hours. Thus the oil of the arachis has the advantage of more than one eighth over olive oil; and it has more or leſs over every other kind of oil. It is an excellent ſubſtitute for olive oil for every domeſtic purpoſe, and is preferable to all other oils for the manufacture of ſoap. The ſeed yields nearly half its weight of oil.

Arachis hypogæa cultivated in France.

Affords an oil excellent for lamps,

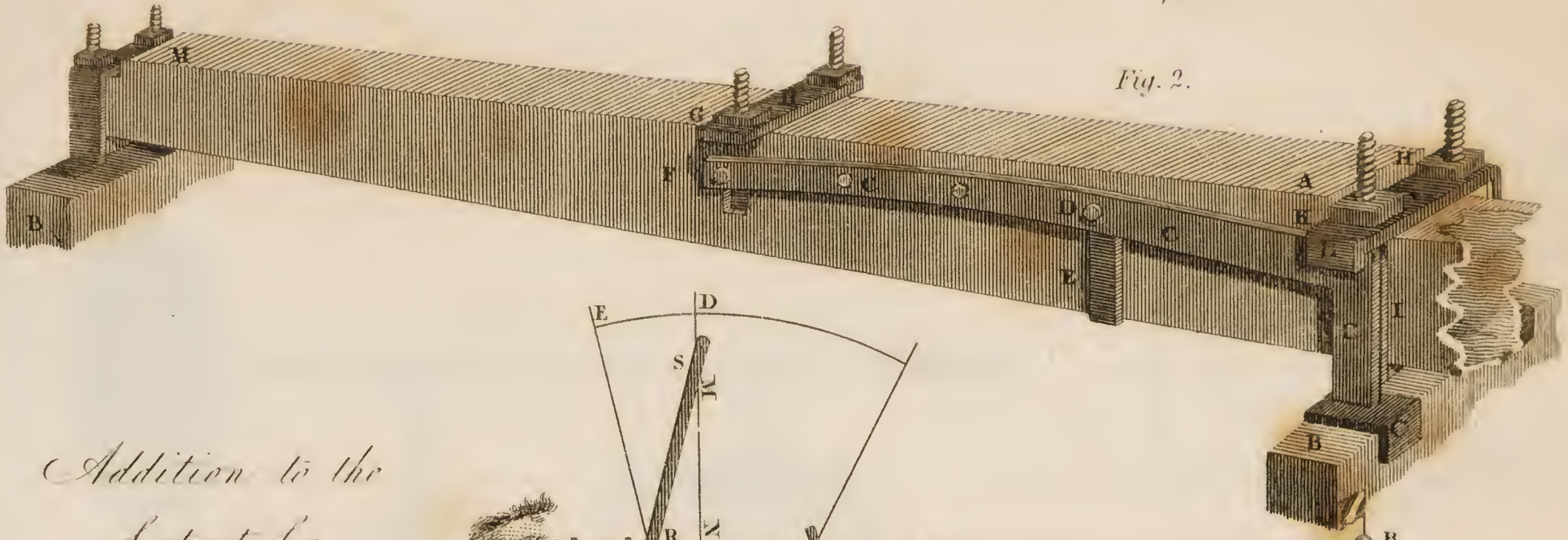
and other purpoſes.

*** The enquiries of *a correſpondent*, reſpecting the method of experiment adopted by Dr. Irvine, will be anſwered in our next.

Drawn by Blunt.

Perspective View of Mr. Woods method of supporting Timbers.

Fig. 2.



Addition to the Sextant for aerostatic observations.

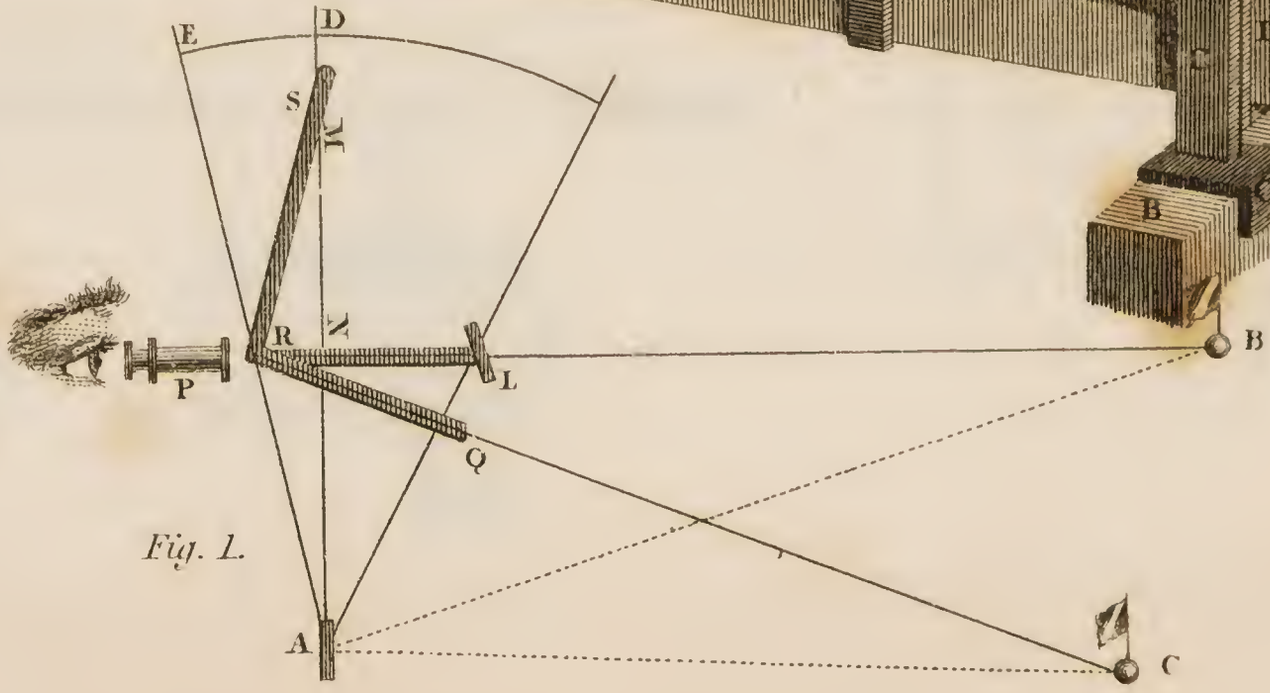
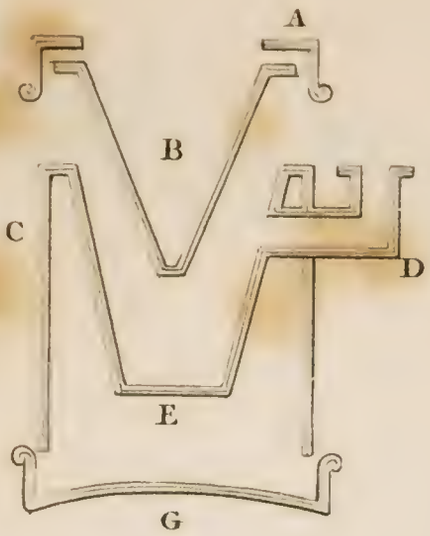
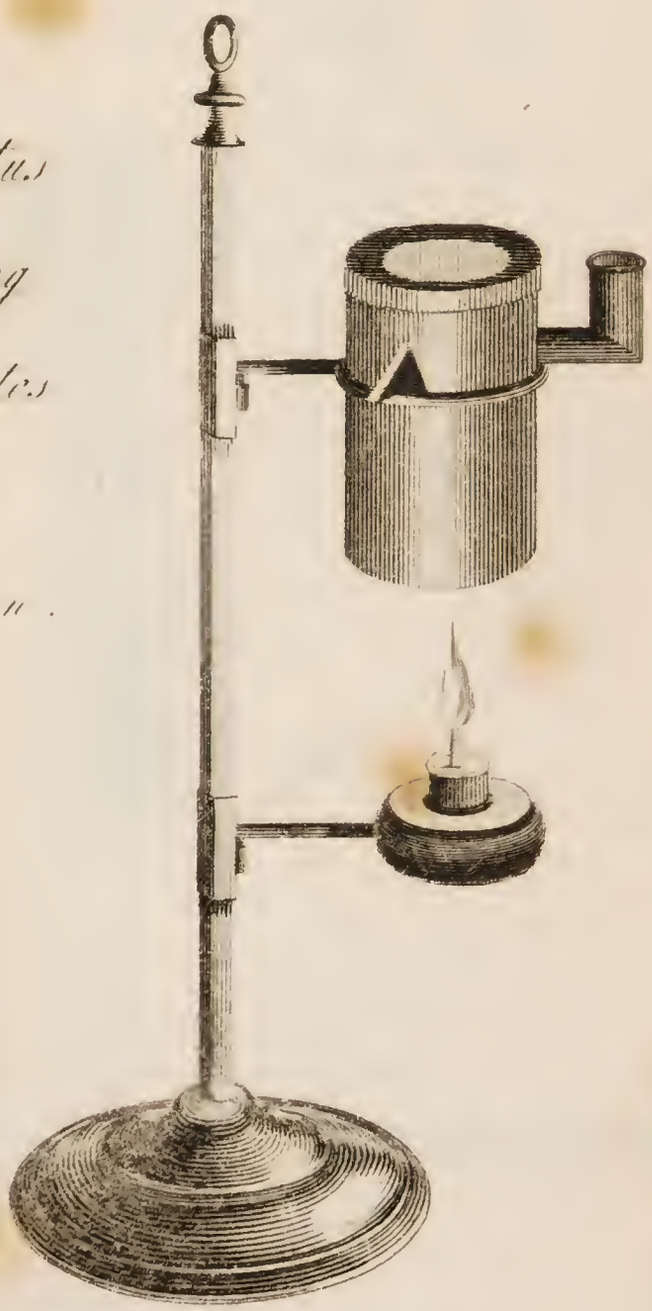


Fig. 1.

Engraved by Mallow Pigott &c.



Apparatus for drying Precipitates and for Congelation.



Mr. Woolf's
Rotatory Apparatus.

Fig. 1.

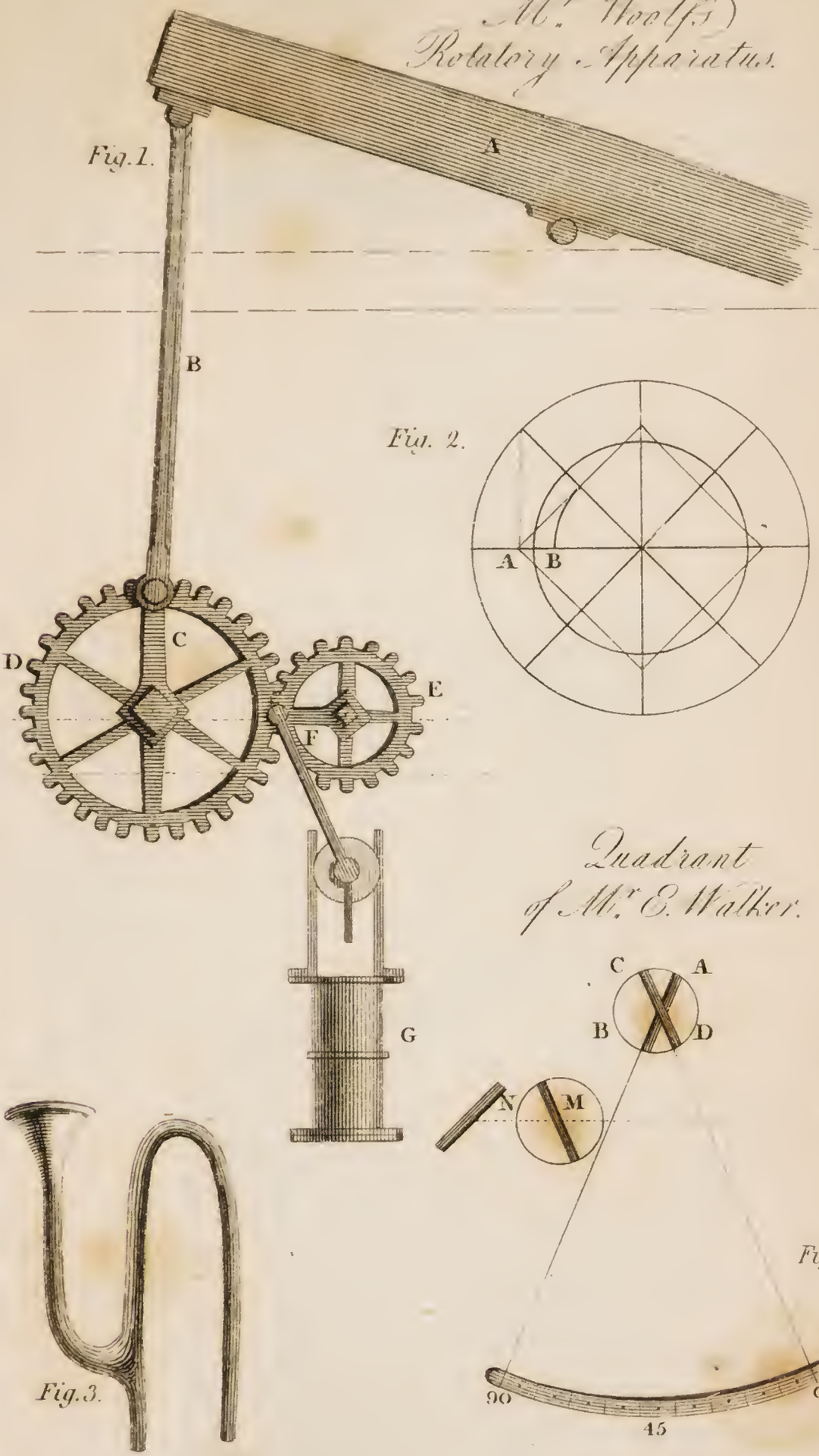


Fig. 2.

Quadrant
of Mr. E. Walker.

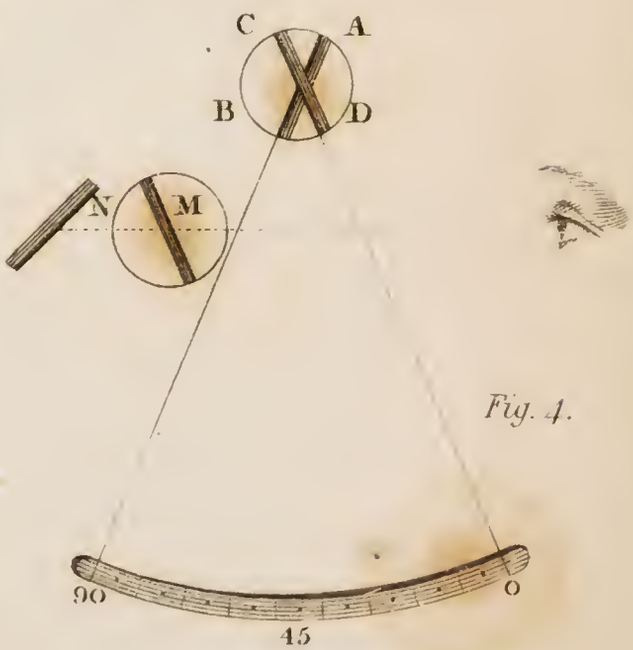


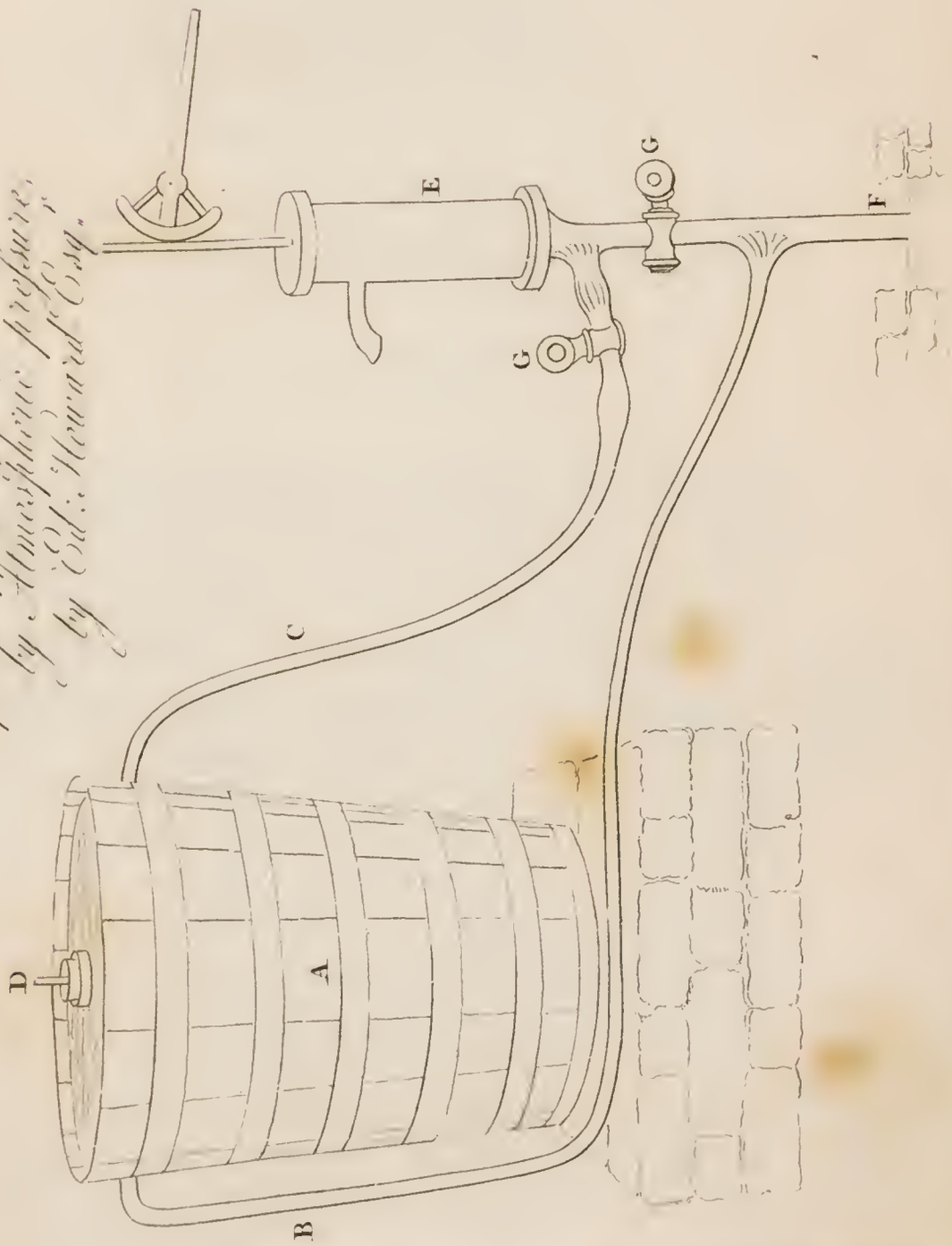
Fig. 4.

Fig. 3.

Improved Syphon.

Dr. Hope's Condiments Apparatus.

Experiment for supplying Steam tubes &c. by Atmospheric pressure, by Edw.d Howard Esq.r



Drawn by Scott.

Engraved by Mutlow Rupsell Gt.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1803.

ARTICLE I.

Letter from ANDREW DUNCAN, *M. D. F. R. S. E. containing Experiments and Obſervations on Cinchona, tending particularly to ſhew that it does not contain Gelatine.*

To Mr. NICHOLSON.

SIR,

HAVING been long engaged in a ſeries of experiments on the aſtringent ſubſtances employed in medicine, I was particularly intereſted with the "Abſtract of a Memoir on the Febrifuge principle of Cinchona," contained in the laſt number of your excellent journal. The preſence of gelatine in cinchona, was ſo incompatible with experiments I had formerly made, that I was ſtrongly inclined to believe, that Seguin (than whom no one ſhould be better acquainted with the combinations of tannin and gelatine) had been miſled, either from having examined cinchona which had been adulterated, or from ſome other accidental cauſe. To ſatisfy myſelf I immediately proceeded to the unerring teſt of experiment, which has convinced me that cinchona does *not* contain gelatine, but ſome other principle not yet ſufficiently examined, which agrees with gelatine, in forming with tannin, a precipitate comparatively inſoluble in water. At the ſame time it is but fair to remark, that my experiments were made with the in-

Preſence of gelatine in cinchona, a miſtake.

But it contains ſome other principle, forming an inſoluble precipitate with tannin.

 fuſion

fuſion and tincture of cinchona, containing all the ſoluble principles of that ſubſtance, whereas Seguin's obſervations are ſaid to be derived from the examination of the iſolated febrifuge principle, of which he gives the following characters: "It precipitates the ſolution of tan, but not the ſolutions of gelatine and ſulphate of iron." On the contrary my experiments teach me, that the entire infuſion and tincture of cinchona, precipitate the ſolution of tan, and alſo the ſolution of gelatine ſlightly, and the ſolution of ſulphate of iron copiouſly. But as the two laſt precipitates may be reaſonably aſcribed to the action of other principles contained in my infuſion and tincture of cinchona, I ſhall not inſiſt upon them, but proceed to ſhew that, although cinchona actually does precipitate the ſolution of tan, yet it does not contain gelatine.

Infuſion and tincture of cinchona, precipitate ſolutions of tan, gelatine, and ſulphate of iron.

Experiment I.

Experiments in proof of this. Infuſion of galls precipitated by infuſion of cinchona.

(*a*) An ounce of infuſion of galls was ſaturated, by adding to it in different portions, an ounce and a half of infuſion of cinchona. The mixture was white and turbid, with a looſe light precipitate.

(*b*) On filtration the fluid paſſed almoſt colourleſs, and perfectly tranſparent.

(*c*) The precipitate when dried, weighed five grains. It had a yellow colour, and an opaque earthy appearance, was extremely friable, and did not adhere to the filtering paper.

Gave a further precipitate with gelatine.

(*d*) The filtred fluid gave no further precipitate with ſolution of cinchona, but with half an ounce of ſolution of gelatine, containing ſix grains of gelatine in each ounce, it produced a copious precipitate, and was ſaturated.

(*e*) The precipitate, when ſeparated by filtration, and dried alſo, weighed five grains, but was hard and brittle, adhered ſtrongly to the paper, had a yellow colour, and exactly reſembled a reſin in appearance.

Experiment II.

Infuſion of galls precipitated by gelatine.

(*a*) An ounce of the ſame infuſion of galls was ſaturated by an ounce and a half of the ſame ſolution of gelatine. Immediately a very copious, whitiſh, tenacious, and adheſive precipitate was formed.

(*b*) On filtration the fluid paſſed very ſlowly, and even after repeated filtration, ſtill retained a ſlight degree of opaline bluiſhneſs.

(*c*) The

(*c*) The precipitate when dried, weighed fourteen grains and a half. It had a browniſh yellow colour, was tranſparent, and had a reſinous appearance and fracture. It was alſo hard and brittle, and adhered ſtrongly to the filter. In every particular it reſembled the precipitate produced in the former experiment (*Exp.* I. *c.*) by gelatine, after the infuſion of galls was completely ſaturated by cinchona.

(*d*) In the filtred liquor (*Exp.* II. *b*) infuſion of cinchona produced no change.

Infuſion of cinchona gave no farther precipitate.

Experiment III.

To an ounce and a half of the ſame infuſion of cinchona, half an ounce of the ſolution of gelatine was added. It produced only a ſlight degree of turbidneſs, and changed the colour of the infuſion from a pale greeniſh to a reddiſh yellow colour. When filtred, it paſſed perfectly tranſparent, and the bottom of the filter was covered with a red varniſh; but it had gained only one grain in weight. In other experiments with larger quantities and ſtronger infuſion of cinchona, the preſence of tannin was more ſtrongly indicated.

Infuſion of cinchona with gelatine gave ſlight ſigns of tannin.

Experiment IV.

Infuſion of galls was not affected by rectified ſpirits of wine, in which iſinglaſs had been long infuſed.

Infuſion of galls not affected by iſinglaſs diſſolved in alcohol.

Experiment V.

(*a*) A tincture of cinchona was prepared by infuſing it in the ſame rectified ſpirits. After it was filtred ſome reſin was ſeparated by precipitation with water and filtration.

Tincture of cinchona precipitated by water,

(*b*) With infuſion of galls this tincture gave a copious precipitate, exactly reſembling that produced by the ſame reagent and infuſion of cinchona. (*Exp.* I. *c.*)

and by infuſion of galls,

Experiment VI.

With tincture of galls the ſame tincture of cinchona gave no precipitate.

Tincture of galls and of cinchona gave no precipitate,

Experiment VII.

In the mixed tincture (*Exp.* VI.) a copious precipitate was produced by diluting it with water.

till diluted by water.

Experiment VIII.

Carbonate of potaſh precipitates ſolution of gelatine,

A ſolution of carbonate of potaſh produced a copious white flaky precipitate in the ſolution of gelatine, which was ſoluble in boiling water, but was not precipitated from the ſolution by infuſion of galls, until ſome acid was added.

Experiment IX.

but not infuſion of cinchona.

The ſolution of carbonate of potaſh changed the colour of the infuſion of cinchona to a fine red, without diſturbing its tranſparency.

Difference between gelatin and cinchonin.

Theſe facts ſeem to me ſufficient to prove the difference between gelatine, and the new principle in cinchona, which for the ſake of convenience, I ſhall venture for the preſent to denominate cinchonin.

Gelatin forms a jelly with water,

Gelatine is ſoluble in water, and the ſolution is diſpoſed to gelatinize. Six grains of iſinglaſs, diſſolved in one ounce of water, form with it at temperatures below 60º Fahrenheit, a jelly of conſiderable firmneſs. From its ſolution in water, gelatine is precipitated by alcohol, and a ſolution of carbonate of potaſh. It is precipitated alſo by tannin, and the precipitates form a hard brown tranſparent maſs.

and is precipitated from it by alcohol, carbonate of potaſh, and tannin.

Cinchonin does nor form jelly with water, is not precipitated by carbonate of potaſh, is ſoluble in alcohol, and combines with tannin.

Cinchonin is ſoluble in water, but gives it no tendency to gelatinize. From its ſolution in water, it is not precipitated by a ſolution of carbonate of potaſh. It is ſoluble in alcohol; it combines with tannin. The componnd is ſoluble in alcohol, but forms, when water is added, or uſed as a menſtruum, a friable opaque yellowiſh precipitate; but cinchonin does not ſeparate even from a watery ſolution of tannin, all that is precipitable by a ſolution of gelatine.

ANDREW DUNCAN, Jun.

Edinburgh, 30th Oct. 1803.

II. *Letter*

II.

Letter from a Correſpondent, containing Diſquiſitions on the Phantaſms of NICOLAI, *and other Derangements of the Animal Syſtem.*

To Mr. NICHOLSON,

SIR,

THE account publiſhed in your laſt number by Mr. Nicolai, of his ſeeing ſpectres, appears not only to admit of an explanation on ſome of the laws of viſion with which we are already acquainted, but may alſo lead to ſome rational account of the grounds, on which the belief in apparitions, which has ſo generally prevailed in all ages of the world, may be founded.

The phantaſms of Nicolai may be explained,

If we look for a moment at the ſun, and afterwards turn our eyes on the blue ſky, or throw them upon the ground, we perceive a black ſpot of the apparent ſize of that luminary. This ſpot will by degrees aſſume a faint green colour, then becomes red; and if we pay attention to theſe phenomena, we ſhall find the green and red colours alternate, becoming gradually more faint, till they wholly diſappear.

—from the impreſſions of viſible objects on the eye.

If the eye be directed for ſome time towards a window, and then covered by the hand, the bars will firſt appear luminous, and the ſquares dark, and then the contrary, and this will alternate till the whole image gradually faints away.

Inſtance of a window.

Theſe are termed ocular ſpectra, and have been ſuppoſed to depend on the alternate tenſion and relaxation of the fibres of which the retina are compoſed. They reſemble the trembling of an over-fatigued muſcle, in which one ſet of fibres attempts to relieve the exertions of another. Theſe phenomena are moſt obvious after the eye has been fatigued by long continued exertion. They are more readily obſerved in the evening than the morning, and the ſpectra of the ſetting are more eaſily caught than of the meridian ſun.

Theſe ocular ſpectra are moſt obvious in the ſtate of debility or fatigue.

In certain ſtates of bodily debility, whether produced by the abſence of volition, or by diſeaſe, they are known ſpontaneouſly to occur. In the former ſtate, which may be termed reverie, when a perſon attends ſlightly to the impreſſions of his ſenſes, without attempting to regulate them by his will, as when a perſon looks careleſsly in the fire during twilight, the infinite

Spontaneous occurrence of ſpectra in the ſtate of reverie.

infinite variety of fantaſtic forms that paſs before the eyes is familiar to every one. Theſe fancies are however, in ſome meaſure, influenced by the train of thinking a perſon is accuſtomed to purſue. Swedenbourg appears to have been perpetually under the influence of this kind of reverie, except when employed in writing an account of what had previouſly occurred to his mind. Had he been conſtantly occupied in active bodily exertion, perhaps it would have diſſipated his phantaſies.

Swedenbourg perpetually in this ſtate.

Why dying perſons pick the bed-clothes, and imagine they ſee demons or dark figures.

Picking the bed cloaths, which appears to occupy the attention of thoſe who are labouring under the debility preceding death, probably ariſes from parts of the retina becoming inſenſible of the impreſſion of light, which produces the ſenſation of ſomething dark lying on the bed-cloaths, which they are deſirous to remove. The enfeebled mind may not unfrequently transform theſe into the figures of demons. The purſuit of the dark ſpot formed by the inſertion of the optic nerve, which in that ſtate is miſtaken for a reality, may alſo give occaſion to this action ſo frequently obſerved among the dying.

Nicolai evidently indulged the ſtate of reverie.

Mr. Nicolai informs us, that he was in the habit of forming vivid repreſentations to himſelf not only of whole ſcenes of comedies, but even of the peculiar dreſſes, forms, and complexions of thoſe who performed them; that is, he accuſtomed himſelf earneſtly to attend to theſe ideal exertions of his own imagination. In the next place he tells us, that he firſt began to ſee ſpectres after having laboured under a nervous fever, and great trouble of mind. For the melancholy caſt of his earlier viſions, or the appearance of dead bodies, his dejected ſtate of mind accounts. That appears however to have ſoon ſubſided. The diſeaſed ſtate of the retina, conſequent to his fever, ſeems to have continued longer.

The phantaſms were of things before ſeen.

This ſtate appears to have been ſuch as to render him ſenſible of the ſpectra of the things he was in the daily habit of ſeeing, ſuch as men, horſes, dogs, &c. for he does not ſay he ever ſaw any thing uncommon. What this particular condition of the retina might conſiſt in, it is impoſſible to determine. It may have been a combination of weakneſs with exceſs of ſenſibility. The ſpectra of the bodies he had ſeen involuntarily recurred to his attention, but not with ſuch ſtrength as to prevent the more forcible impreſſion of what was in reality paſſing before

before him. The notion of hearing the phantoms ſpeak I ſhould refer to an affection of the organ of hearing, ſimilar to what took place in the eye. It is evident they were both removed by a ſlight diminution of the quantity of circulating blood. Audible deluſions.

Many ſtories of apparitions may, in my opinion, be accounted for on ſimilar principles. A perſon fixes his eyes intently on the face of an expiring friend illuminated by the light of a candle, perhaps with the intention of taking a laſt farewell; ſoon after, going into the dark, the ſpectrum of this luminous appearance occurs to the fatigued eye, and he thinks he perceives the dying man he had juſt left, ſtanding before him. Phantaſms of departed friends.

On theſe principles we may venture to correct an error in the general repreſentation of our juſtly popular play of Hamlet. The ghoſt ſhould only appear once. This ſingle appearance makes ſo ſtrong an impreſſion on the mind of Hamlet, which, together with an habitual melancholy, was debilitated by care and vexation, that whenever afterwards he thinks ſeriouſly on his father, the ſpectrum of the ghoſt recurred to his eye, as he himſelf informs us, when he tells Horatio he ſees his father, and is aſked where, he ſays, "*in my mind's eye.*" Correction of the repreſentation of Hamlet.

Theſe curſory remaks, Sir, are in ſome meaſure written to evince how much better it is to attempt at leaſt, to account for phenomena on principles already known, than to hunt for a new cauſe for every uncommon appearance. But what kind of philoſophiſing can we expect from a man, who quotes ſuch canons as the following, for rules of philoſophy? "That knowledge derived from experience is merely empirick, and therefore not to be depended on." "That obſervation ſhould not be admitted in theoretical philoſophy." What is theoretical philoſophy? After the exiſtence of Bacon, of Newton, and of Locke, who could have expected to live to ſee the whole thinking part of a nation puzzling themſelves about opinions, which, if they admit of any deſcription, may be characterized as a jumble of the abſtractions of Ariſtotle, with the ideal ſyſtem of Berkley? Concluding remark.

A STUDENT.

III. *Ex-*

III.

Experiments on the Subſtance vulgarly called Gum Kino. By CIT. VAUQUELIN *.

Kino not a gum.

THE name given to this ſubſtance is by no means ſuited to it; and were it not a common practice, to give names to things before we are acquainted with their nature, it would be inconceivable how it ſhould have been called a gum, having neither the phyſical nor chemical properties of one.

Whence it is obtained not known.

Neither have we any accurate knowledge of the tree or of the country that produces it; but it appears to have been firſt brought to Europe by the Engliſh, who made known its medicinal properties, and introduced it into our ſhops.

Said to be from the pau de ſangue of Africa.

It is called in trade *kino* or the *gum-reſin* of Gambia. Dr. Oldfield, who made it known to Fothergill, termed it the true gum of Senegal. In the Medical Obſervations and Inquiries, it is ſaid to be brought from Africa, and the tree that furniſhes it to be called by the natives *pau de ſangue*.

Uſed in medicine as a tonic.

As a medicine it is uſed in the form of boluſſes, lozenges, aqueous infuſion, and ſpirituous tincture, as a tonic and aſtringent.

Yields an aqueous fluid, an oil, carbonic acid, and carbonated hidrogen.

Subjected to the action of fire, it melts and ſwells up conſiderably: yields at firſt a clear liquor, which in a few inſtants becomes coloured; a light and nearly white oil then paſſes over, which in the courſe of the proceſs becomes coloured and heavier than the aqueous product. A ſmall quantity of carbonic acid is likewiſe formed, with a large quantity of carbonated hidrogen gas.

Its oil.

The oil produced in this operation unites with cauſtic fixed alkalis, and forms a deep red liquor, that becomes of a dull green on expoſure to the air.

Its aqueous product.

The aqueous product is not acid, but has an acrid burning taſte, owing to a portion of the oil retained in ſolution; and potaſh ſeparates from it a large quantity of ammoniac.

Its reſiduum.

Twenty grammes diſtilled with a ſtrong heat left eight and half of a very bulky coal, marked with the colours of the rainbow; and this coal afforded ſeventy-two centigrammes of aſhes, conſiſting chiefly of lime, ſilex, alumin, and oxide of iron.

* Abridged from the *Annales de Chimie*.

Kino

Kino is little ſoluble in cold water, but much more in hot, though a portion of it is inſoluble. The ſolution is ſlightly acid: alcohol does not precipitate it, but ſeparates ſome reddiſh flocks; when made with boiling water it grows turbid on cooling, and depoſits a brown red precipitate. Its ſolution in water acidulous.

A ſaturated ſolution is precipitated by mild alkalis, but water in ſufficient quantity re-diſſolves the precipitate. Precipitated by mild and cauſtic alkalies,

Cauſtic alkalis likewiſe precipitate it, but if added in exceſs re-diſſolve the precipitate.

Glue diſſolved in water forms a very conſiderable roſe-coloured coagulum with the ſolution of kino; and if the quantities be ſuch, that both ſubſtances are ſaturated, the ſupernatant fluid will be nearly colourleſs. and coagulated by glue.

Though theſe appearances indicate the preſence of tannin in kino, it does not precipitate ferruginous ſalts black, but of a beautiful deep green, ſcarcely alterable by expoſure to the air. This property it has in common with the infuſions of cinchona and rhubarb; whence it is probable, that theſe three ſubſtances contain a principle of ſimilar nature. Its tannin precipitates iron green. Contains a principle common to i , bark, and rhubarb.

This principle, whatever it be, is very deſtructible: for, if a little oxigenated muriatic acid be poured on the precipitate it forms with iron, this loſes its colour, and does not re-appear on the addition of an alkaline carbonate, which produces only a red oxide of iron. This principle very deſtructible.

The ſolution of kino copiouſly precipitates acetite of lead of a yellowiſh grey; nitrate of ſilver of a reddiſh yellow, and tartrite of antimony of a yellowiſh white, but much more copiouſly than the infuſion of tan or of cinchona; which ſeems to indicate, that it would be a better antidote in caſes of perſons poiſoned by this metallic ſalt. Precipitates ſome metallic ſolutions. Uſeful as an antidote.

Wool and cotton being boiled in a ſolution of kino, and then dipped in a bath of ſulphate of iron, appeared on immerſion of a bottle-green; but being waſhed and dried, the colour became a blackiſh brown. It was very durable. Dyes wool and cotton a blackiſh brown.

Hot alcohol diſſolves kino very well, all but a ſmall portion. Water renders the ſolution a little turbid, but precipitates nothing. Diſſolves in hot alcohol.

The portion inſoluble in alcohol, nearly a fourth of the whole, has neither the bitterneſs nor aſtringent taſte of kino; but, on the contrary, is rather mucous and ſweet. It eaſily diſſolves Contains a portion of gum.

in

in hot water, and gives it a fine red colour. It is precipitable by alcohol; but neither by glue, nor by any metallic ſolution. On burning, it diffuſes a ſmell reſembling that of gum.

This favours the ſolution of the other principle in water.

I ſuſpect the preſence of this ſubſtance favours the ſolution in water of the principle ſoluble in alcohol; for the latter is leſs ſoluble in water when ſeparated from the former; and, if the quantity of water neceſſary for diſſolving the aſtringent part be not employed in the firſt inſtance, what is left requires a greater proportion of water.

Water diſſolves $\frac{4}{5}$, alcohol moſt of the remainder.

Four litres of water, uſed at different times, left near twenty grammes out of a hundred of kino undiſſolved. The reſiduum grew ſoft like a reſin in boiling water, and all of it, except ſeven decigrammes, was ſoluble by alcohol, to which it imparted all the properties before obſerved in the aſtringent matter.

Sulphuric acid renders it leſs ſoluble.

Sulphuric acid diminiſhes the action of water on kino, inſtead of increaſing it, as it does with reſpect to the reſinous part of cinchona.

It tans leather.

It is capable of being uſed for tanning leather.

Neither a gum, nor a gum-reſin, but chiefly a ſpecies of tannin.

From what has been ſaid it appears, that the greater part of kino conſiſts of tannin, and is neither a gum, nor a gum-reſin. But there is a ſlight difference between it and the tannin of galls and oak bark, which precipitate iron of a blue black, while kino precipitates it green, in which it reſembles cinchona and rhubarb. If therefore it were to become plentiful and cheap, it might be employed for all the purpoſes for which aſtringent vegetables are commonly uſed.

Addition.

Dr. Duncan firſt aſſerted it to be an extract.

Mr. Vauquelin is not the firſt who diſcovered the common error reſpecting kino. In the new Edinburgh Diſpenſatory, Dr. Duncan has entered pretty fully into the ſubject, and aſſerts it to be in reality an extract. He adds, that what we have now in the ſhops is not brought from Africa, but chiefly from Jamaica. In a private letter he informs me, that this is an extract of the *coccoloba urifera*, or ſea-ſide grape; while the fineſt kino of the ſhops, and what from ſome circumſtances he ſuppoſes was the ſort analyzed by Vauquelin, is the product of different ſpecies of *eucalyptus*, particularly the *reſinifera*, or brown gum tree of Botany Bay, from which country a parcel was imported ſome years ago.

Obtained from the ſea-ſide grape,

and gum-tree of Botany Bay.

W. N.

IV.

Extract of a Letter from DR. PRINCE, *reſpecting his Air-Pump.*

IT is unneceſſary here to enter into the merits of Dr. Prince's air-pump, as they were noticed in the 1ſt. Vol. of the Philoſophical Journal, 4to. edit. p. 130. The purpoſe of his letter is to defend himſelf againſt the miſrepreſentations of the writer of the article pneumatics, in the Encyclopedia Britannica; which he has done in the ſupplement to the edition of that work reprinting in America, where it may be preſumed it will meet the eye of but few Engliſh readers.

Dr. Prince's air-pump miſrepreſented in the Encyclopedia Britannica.

The Encyclopedia ſays, "great inconveniences were experienced from the oſcillations of the mercury in the gauge. As ſoon as the piſton comes into the ciſtern, the air from the receiver immediately ruſhes into the barrel, and the mercury ſhoots up in the gauge, and gets into a ſtate of oſcillation. The ſubſequent riſe of the piſton will frequently keep time with the ſecond oſcillation, and increaſe it. The deſcent of the piſton produces a downward oſcillation, by allowing the air below it to collapſe; and by improperly timing the ſtrokes, this oſcillation becomes ſo great, as to make the mercury enter the pump."

Charged with inconvenience from oſcillations of the mercury in the gauge.

This, Dr. Prince obſerves, is a very ſingular account of the working of the American air-pump. It *ſeems* to be founded on experiment, yet it is contradicted by numerous experiments performed with the original pump, and with one on the ſame conſtruction, made by the late Mr. G. Adams in London. Many ſcientific and reſpectable perſons were witneſſes, that no ſuch extraoſcillations were produced by it; but that the mercury roſe in the gauge in the ſame manner as it did in a double-barrelled pump of the common conſtruction made by Nairne. Add to this, Mr. Adams, who made the firſt pump in England on this plan, mentions no ſuch effect, either in his letter to the inventor, or in his public account of it; nor does Mr. Jones, who has ſince made pumps on this plan, and given an account of their exhauſting power, which he ſays, in a letter to Dr. Prince, is fully equal to that of Cuthbertſon's.

This contradicted by facts.

To

Valves ſaid to be put in the piſton to prevent downward oſcillation;

To prevent the downward oſcillation, which, Dr. Prince obſerves, could only occur in a ſingle-barrelled pump, and which he obviated by uſing two barrels, the Encyclopedia ſays, "valves were put into the piſton; but as theſe require force to open them, the addition ſeemed rather to increaſe the evil, by rendering the oſcillation more ſimultaneous with the ordinary rate of working." If, replies the Doctor, ſuch an evil were produced by the deſcent of the piſton, it is difficult to conceive, how putting valves into the piſton could have increaſed it. They could not increaſe the evil, unleſs they increaſed the reſiſtance to the air under the piſton. But it muſt be a ſtrange aſſertion, that a piſton with a *valve* in it will give more reſiſtance to the air than a *ſolid* piſton.

but valves could not increaſe its reſiſtance.

Said to be difficult of execution:

Another objection is: "it appears of very difficult execution. It has many long, ſlender, and crooked paſſages, which muſt be drilled through broad plates of braſs, ſome of them appearing ſcarcely practicable. It is rare to find plates and other pieces of braſs without air-holes, which it would be difficult to find out and cloſe, &c. Now the fact is, there is not ſo much pipe and duct-work in the American air-pump, by more than one half, as in Mr. Nairne's improved pump of Smeaton, againſt which no ſuch objection is brought. There can be no reaſon to preſume the braſs work would be more defective, unleſs it were more complex in its form. And it is obviouſly far ſuperior in point of ſimplicity to Cuthbertſon's, which the Encyclopedia conſiders as "the moſt perfect that has yet appeared."

but leſs ſo than others.

After theſe ſtrictures on the Encyclopedia Britannica, Dr. Prince gives an account of the improvements he has made on his former attempt.

Subſequent improvements.

'The following improvements have been made in the American air-pump, by the inventor, to render it more ſimple and convenient. It has been obſerved above, that in all air-pumps, made to condenſe as well as exhauſt by the ſame barrels and winch, there muſt be additional pipes, ducts and cocks to command and regulate the operations: But this is not the beſt method of conſtructing the inſtrument for exhauſting and condenſing experiments: for a great ſtrain is brought upon the rackwork of the pump when ſeveral atmoſpheres are throw into the receiver: and the pump may be made with leſs trouble and expenſe by fixing a common condenſing

denſing ſyringe to it, in the following manner. Let a ſtraight pipe be fixed to the ciſterns, and paſs horizontally to the receiver-plate, as in the common table air-pump. At a convenient diſtance from the barrels this pipe muſt be ſwelled out ſo as to admit the key of a ſtop-cock. The key of this cock muſt be pierced quite through in the direction of its handle; and half way through, at a right angle to meet the other hole. A ſmall pin muſt be fixed in the handle, on that ſide which correſponds with the ſhort hole. A hole muſt be made in the ſide of the pipe to correſpond occaſionally with the holes in the key. This cock is more ſimple than the one in the original pump, and will regulate the exhauſting and condenſing experiments. To ſet the cock for exhauſting the receiver, bring the handle of the key parallel with the pipe, with the ſolid part of the key againſt the hole in the ſide of the pipe; then will the communication be opened between the barrels and receiver, and the receiver may be exhauſted. To reſtore the equilibrium, or let the air into the receiver, ſet the handle of the key at right angles with the pipe, and let its projecting pin point to the receiver; then will the communication be opened between the atmoſphere and receiver, through the hole in the ſide of the pipe and the cock. In this ſituation the ſolid part of the key will cloſe the paſſage in the pipe leading to the barrels. If a condenſer, having a valve at its end, be now attached to the ſide of the pipe, oppoſite the hole, the air may be forced into the receiver through the cock without entering the barrels. The ſwelled part of the pipe, in which the key is inſerted, ſhould be ſo made as that the condenſer may be ſcrewed on or off, at pleaſure. The equilibrium may be reſtored in the receiver, either by unſcrewing the condenſer a little, or by letting the air out through the barrels.

Beſt made to condenſe by means of a ſyringe.

Stop-cock for regulating the experiments.

Mode of condenſing.

'In this conſtruction, the pipe ſtanding between the barrels in the original pump, and the drilled paſſages in the horizontal piece connecting this pipe with the regulating cock, are unneceſſary. The pump is rendered more ſimple, and every difficulty of execution on account of crooked paſſages, &c. removed. This alteration in the American air-pump was contrived by its inventor, and a table-pump made on this plan, for him, by the late Mr. G. Adams, before the laſt edition of the Encyclopedia was printed.

The pump thus rendered more ſimple.

'Another

Alteration in the valve-pump.

'Another alteration, ſince made, is in the ſituation of the valve-pump: the laſt mentioned pump not having one fixed to it. In all air-pumps having the tops of the barrels cloſed with plates and collars of leather, as in Nairne's, Cuthbertſon's, and the American pump (as now altered by removing the middle pipe,) it is neceſſary to connect oil boxes with the top-plates to receive the oil which is thrown out of the barrels in working the pump. Cuthbertſon's pump has two, one to each barrel. By removing the pipe from between the barrels, in the Americen pump, a ſmall barrel is ſcrewed in its place to the croſs-piece, which connects the top-plates covering the valves. The barrel anſwers the purpoſe of an oil-box in common exhauſtions. When greater vacuums are wanted in the receiver, this barrel anſwers alſo for a valve-pump. On the top of the croſs-piece is ſcrewed a collar of leathers containing a piſton and its rod, to work occaſionally in the barrel below. At the lower end of the barrel is a valve covered with a cap: by unſcrewing the cap, and paſſing down the piſton, all the oil in the barrel is expelled through the valve; and afterwards the barrel, and the ſpace above the valves on the top-plates of the great barrels, are exhauſted of air, by working this ſmall pump. The ſmall piſton when drawn up to its collar of leathers is above the holes in the croſs-piece leading from the valves. When the ſmall barrel is uſed only as an oil-box, the collar of leathers, with the piſton, is removed, and a button, with a ſhort pipe in it, ſcrewed in its place to give vent to the air when expelled from the barrels: In this valve-pump there is not ſo much work as in Cuthbertſon's two oil-boxes; nor is it an additional expence; for the ſyringe, which is uſed with the lead weight in the receiver, is made to ſcrew to the croſs-piece for this purpoſe; the weight being taken off, and a cap ſcrewed on over the valve, when uſed as an oil-box. In the collars of leathers, on the tops of the barrels, are put two ſmall flat boxes, below one or two rings of the leathers, the piſton rods paſſing through them. Theſe boxes contain the oil to keep the leathers moiſt, and air-tight. In this ſituation the oil is not thickened by evaporation, nor carried up from off the leathers, when the piſton riſes, as in Nairne's pump, and the leathers are better ſupplied than by the dirty oil paſſing through the pump and returned to the collars by Cuthbertſon's crooked pipes. The American air-pump, made in this manner, is the ſimpleſt form of any pump of equal power.'

Barrel to anſwer the purpoſe of an oil-box, and alſo a valve-pump.

This no additional expence.

The oil neither thickened by evaporation, nor carried off from the leathers.

M em air

V.

Memoir on the Tides. By Cit. LAPLACE *.

THE object of this paper is to compare the high tides obſerved on the 23d of March laſt, with the reſults indicated by the theory of univerſal gravitation.

At this period the moon was new, and in her perigeum. Theſe circumſtances, joined to thoſe of an equinoctial ſyzygy, are the moſt favourable to high tides: and if at ſuch a time the action of the winds ſhould combine with theſe regular cauſes, inundations may follow, againſt which it is prudent to uſe precautions. It is with this view that the board of longitude publiſhes in the *Connoiſſance des Tems* every year, a table of the higheſt tides that follow every new and full moon.

New moon, in perigeum, and equinoctial ſyzygy, moſt favourable to high tides.

To know the real height of the tide produced by the action of the ſun and moon, and diſtinguiſh it from that which is owing to the temporary action of the wind, it is not ſufficient to obſerve the abſolute height at flood, but the correſpondent ebb muſt be obſerved likewiſe, and the difference between them gives the total height of the tide. We can eaſily conceive, that the action of the wind muſt add to the height of the water, both at ebb and flood, nearly in an equal degree. This conſideration is indiſpenſable, for without it all we can deduce from obſervation is the ſum of the combined effects, without being able to ſeparate and refer them to their real cauſes.

Height of tide the difference between ebb and flood.

Wind acts equally on both.

The tides of the 23d of March were obſerved at Breſt by Cit. Rochon and Mingon. The total height was 23 feet four inches, the greateſt ever obſerved. That which came neareſt to it, was as far back as the 23d of September 1714, when the moon was full, in her perigeum, and almoſt without declination, as well as the ſun. Its total height was 22 feet 11 inches.

Tide of 23d of March.

Sept. 23, 1714.

According to the theory given in the fourth book of *La Mécanique Céleſte*, the greateſt difference between high and low water in the preceding ſyzygies, is 22 feet 10 inches, which differs very little from the obſervations: But in that book it is remarked, that the local circumſtances of every harbour may occaſion the relation of the action of the ſun and moon on the

Theory in *La Mecanique Celeſte* agreeable to obſervation.

* National Inſtitute of France, IV.

phenomena of the tides to vary. A compariſon of the obſervations made at Breſt, has made known to Cit. Laplace, that circumſtances there increaſe the action of the moon one-ſixth; and with this modification the reſult of the theory is a mean between thoſe given by obſervation.

Time of high-water at Breſt has not varied in near a century.

The high tide of the 15th of September 1715, in the morning, and that of the 23d of March laſt, in the evening, were nearly equidiſtant from the ſyzygy, which ſhould give the ſame hour for the tides, if the local circumſtances of the harbour have not varied in the interval of nearly a century. The firſt was obſerved at half after four in the morning, true time; the ſecond, at 29 minutes after four in the evening: whence it appears, that the time of the tides at Breſt has not varied in that period.

Series of obſervations on the tides propoſed.

Cit. Laplace has propoſed to the firſt claſs of the Inſtitute, to ſolicit government to direct a ſeries of obſervations to be made on the tides in the different harbours of France; and to appoint a committee to draw up a ſingle body of inſtructions for the beſt mode of making theſe obſervations. Both propoſals were adopted.

The whole of the paper, of which an abſtract is here given, will be printed in the *Connoiſſance des Tems*.

VI.

Abſtract of a Paper by Cit. GUYTON-MORVEAU, *entitled an Examination of a native Carbonate of Magneſia*.*

Magneſia with carbonate generally in ſmall quantity in ſtones.

THOUGH magneſia is a conſtituent part of many ſtones, it enters into them but in ſmall quantity, with few exceptions. Native carbonate of magneſia occurs ſtill more rarely in any conſiderable proportion. Citizen Guyton, however, ſearching for a clay poſſeſſed of the hygrometric property in the higheſt degree, lately found a very large proportion of native carbonate of magneſia in a ſtone in the vicinity of Caſtella-Monte, which is there conſidered as a clay very rich in alumine.

Characters of the ſtone of Caſtella Monte.

This ſtone is as compact as the hardeſt chalk, in an amorphous maſs, and as white as ceruſe. It does not ſenſibly adhere to the tongue, and has no argillaceous ſmell. Water acts

* Bulletin des Sciences, No. 75.

very

very ſlightly on it; it is not reducible to a ſolid paſte; yet on drying it appears to agglutinate, and contract a little its dimenſions. Its ſpecific gravity, when all the bubbles of air it contains have eſcaped, is 2.612. In the fire it loſt 0.585 of its weight, and became ſufficiently hard to ſcratch Bohemian glaſs ſlightly. Five grammes, being diſſolved in nitric acid, gave out a large quantity of gas, by which the weight was diminiſhed 230 centigrammes.

Concentrated ſulphuric acid, poured on the ſtone reduced to powder, occaſioned a violent efferveſcence on the addition of water. Without this addition the effect was not perceptible. Efferveſces with ſulphuric acid,

By this treatment a ſalt cryſtallized in ſmall needles was obtained, which diſplayed all the properties of ſulphate of magneſia. and yields ſulphate of magneſia.

This ſalt was precipitated by potaſh, and the precipitate, when dried, weighed 131.5 centigrammes.

The portion not diſſolved by ſulphuric acid was pure ſilex, and weighed 71.2 centigrammes. Contains ſilex.

Pruſſiat of ſoda gave the ſolutions a ſlight greeniſh tinge, but nothing capable of being weighed was depoſited.

This ſtone therefore contains, Its analyſis.

Magneſia	26.3
Silex	14.2
Carbonic acid	46
Water	12
Iron	0
Loſs	1.5
	100.0

Cit. Guyton obſerves, that the difference between the proportions of the conſtituent ſubſtances of this ſtone, and thoſe of the artificial carbonate of magneſia, ariſes no doubt from the circumſtances in which theſe ſalts have been produced; and the other characteriſtics, that diſtinguiſh them, may be occaſioned by the preſence of the other ſubſtances found with the carbonate of magneſia in the ſtone of Caſtella-Monte. How it differs from the artificial carbonate.

VII.

Curious Particulars reſpecting the Mountains and Volcanos, and the Effect of the late Earthquakes in South America, with Remarks on the Language and Science of the Natives, and other Subjects. By M. A. Von Humboldt *.

Three branches of the Andes.

We arrived at Quito, by croſſing the ſnows of Quridien and Tolima, for as the cordillera of the Andes forms three ſeparate branches, and at Santa Fe de Bogoto, we were on the eaſternmoſt, it was neceſſary for us to paſs the loftieſt, in order to reach the coaſt of the Pacific ocean. We travelled on foot, and ſpent ſeventeen days in theſe deſerts, in which are to be found no traces of their ever having been inhabited. We ſlept in huts made of the leaves of the heliconia, which we carried with us for the purpoſe. Deſcending the Andes to the weſt, there are marſhes, in which you ſink up to the knees. The latter part of the time we were deluged with rain; our boots rotted on our legs; and we arrived barefoot at Carthago, but enriched with a fine collection of new plants, of which I have a great number of drawings.

Huts made of the leaves of heliconia.

The Andes marſhy toward the weſt.

Mines of Platina in mount Choca.

From Carthago we went to Popayan, by way of Buga, croſſing the beautiful vale of the river Cauca, and having conſtantly at one ſide the mountain of Choca, in which are the mines of Platina.

Baſaltic mountains.

Volcano of Purace.

We ſtaid during the month of November 1801, at Popayan, viſiting the Baſaltic mountains of Juluſuito; the mouths of the volcano of Purace, which evolve, with a dreadful noiſe, vapours of hydroſulphurated water; and the porphyritic granites of Piſche, which form columns of five, ſix, or ſeven ſides, ſimilar to thoſe I remember I ſaw in the Euganean mountains in Italy, which Strange has deſcribed.

Columnar porphyritic granites.

Paramos piercing cold and deſtitute of vegetation.

In travelling from Popayan to Quito, we had to croſs the *paramos* of Paſto, and this in the rainy ſeaſon. Every place in the Andes, where, at the height of 3500 or 4000 yards, vegetation ceaſes, and the cold penetrates to the very marrow of your bones, is called a *paramo*. To avoid the heats

* Abridged from the Magazin Encylopédique.

of

of the valley of Patia, where, in a ſingle night, a fever may be caught, that will laſt three or four months, we paſſed the ſummit of the Cordillera, traverſing frightful precipices.

We ſpent our Chriſtmas at Paſto, a little town at the foot of a tremendous volcano, where we were entertained with great hoſpitality. The roads leading to and from it are the moſt ſhocking in the world. Thick foreſts, between marſhes, in which the mules ſink up to their bellies; and gullies ſo deep and narrow, that we ſeemed entering the galleries of a mine. Town of Paſto.

The whole province of Paſto, including the environs of Guachucal and Tuqueres, is a frozen plain, nearly beyond the point where vegetation can ſubſiſt, and ſurrounded by volcanos and ſulphur-pits, continually emitting volumes of ſmoke. The wretched inhabitants of theſe deſerts have no food but potatoes: and if theſe fail, as they did laſt year, they go to the mountains to eat the ſtem of a little tree, called *achupalla (pourretia pitcarnia)*; but the bears of the Andes, as they too feed on it, often diſpute it with them. On the north of the volcano of Paſto, I diſcovered, in the little Indian village of Voiſaco, 1900 yards above the level of the ſea, a red porphyry, with baſe of argil, encloſing vitreous feldſpar, and hornblende, that has all the properties of the ſerpentine of the *Fichtelgebirge*. This porphyry has very diſtinctly marked poles, but no attractive power. Near the town of Ibarra, we nearly eſcaped being drowned by a very ſudden ſwell of the water, accompanied with ſhocks of an earthquake. The province a frozen plain. The people live on potatoes, and the ſtems of the pouretia pitcarnia. Red porphyry with diſtinct poles.

We reached Quito on the 6th of January 1802. It is a handſome city; but the ſky is commonly clouded and gloomy. The neighbouring mountains exhibit little verdure, and the cold is very conſiderable. The great earthquake on the 4th of February 1797, which changed the face of the whole province, and in one inſtant deſtroyed thirty-five or forty thouſand perſons, has ſo altered the temperature of the air, that the thermometer is now commonly 41° to 54°, and ſeldom riſes to 68° or 70°, whereas Bouguer obſerved it conſtantly at 66° or 68°. Since this cataſtrophe, earthquakes are continually recurring; and ſuch ſhocks! it is probable, that all the higher ground is one vaſt volcano. What are called the mountains of Cotopoxi and Pichincha, are but little Quito. Earthquake of 1797. altered the climate greatly. The heights one vaſt volcano.

ſummits, the craters of which, form different conduits terminating in the ſame cavity. The earthquake of 1797, afforded a melancholy proof of this; for the ground then opened every where, and vomited forth ſulphur, water, &c. Notwithſtanding the dangers and horrors that ſurround them, the people of Quito are gay, lively, and ſociable, and in no place did I ever ſee a more decided and general taſte for pleaſure, luxury, and amuſement. Thus man accuſtoms himſelf to ſleep tranquilly on the brink of a precipice.

People of Quito.

Pichincha.

I was twice at the mouth of the crater of Pichincha, the mountain that overlooks the city of Quito. I know of no one but Condamine, that ever reached it before; and he was without inſtruments, and could not ſtay above a quarter of an hour, on account of the extreme cold. I was more ſucceſsful. From the edge of the crater riſe three peaks, which are free from ſnow, as it is continually melted by the aſcending vapour. At the ſummit of one of theſe I found a rock, that projected over the precipice, and hence I made my obſervations. This rock was about twelve feet long, by ſix broad, and ſtrongly agitated by the frequent ſhocks, of which we counted eighteen in leſs than half an hour. We lay on our bellies, the better to examine the bottom of the crater. The mouth of the volcano forms a circular hole, near a league in circumference, the perpendicular edges of which are covered with ſnow on the top. The inſide is of a deep black; but the abyſs is ſo vaſt, that the ſummits of ſeveral mountains may be diſtinguiſhed in it. Their tops ſeemed to be ſix hundred yards below us, judge then where their baſes muſt be. I have no doubt but the bottom of the crater is on a level with the city of Quito. Condamine found it extinct, and even covered with ſnow; but we had to report the unpleaſant news, that it was burning. On my ſecond viſit, being better furniſhed with inſtruments, I found the diameter of the crater to be 1600 yards, whereas that of Veſuvius is but 670. The height of the mountain is 5280 yards.

Its crater.

Several mountains within it.

Its diameter 1600 yards;

height 5280.

When we viſited the volcano of Antiſana, the weather was ſo favourable, that we reached the height of 5915 yards. In this lofty region, the barometer ſunk to 14 inches 7 lines, [15.6 Eng.] and the tenuity of the air occaſioned the blood to iſſue from our lips, gums, and even eyes: we felt extremely feeble, and one of our company fainted away. The

Volcano of Antiſana, 5915 yards.

Barometer at 15.6 inches.

Hemorrhages brought on.

air

air brought from the loftieſt point we viſited, gave on being analyſed 0.218 of oxigen gas, and 0.008 of carbonic acid. Air 0.218 of oxigen.

We viſited Cotopoxi, but could not reach the mouth of the crater. The aſſertion, that this mountain was diminiſhed in height by the earthquake of 1797, is a miſtake. Cotopoxi not ſunk by the earthquake of 1797.

In June we proceeded to meaſure Chimboraco and Tunguragua, and take a plan of all the country affected by the grand cataſtrophe of 1797. We approached within about 500 yards of the ſummit of Chimboraco, our aſcent being facilitated by a line of volcanic rocks bare of ſnow. The height we reached was 6465 yards; and we were prevented from aſcending farther by a chaſm too deep to croſs. We felt the ſame inconveniences as on Antiſana; and were unwell for two or three days after. The air at this height contained 0.20 of oxigen. The trigonometrical meaſurement I took of the mountain at two different times, and I can place ſome confidence in my operations, gave me for its height 6970 yards, a hundred more than Condamine aſſigns it. The whole of this huge maſs, as of all the high mountains of the Andes, is not granite, but porphyry, from the foot to the ſummit, and there the porphyry is 4050 yards thick. Air at 6465 yards, contained 0.20 of oxigen. Chimboraco 6970 yards high. Conſiſts of porphyry.

Chimboraco is probably a volcanic mountain, for the track by which we aſcended, conſiſts of a burnt and ſcoriſied rock mixed with pumice-ſtone, reſembling all the ſtreams of lava in this country, and ran higher up the mountain than we could climb. The ſummit therefore is in all likelihood the crater of an extinct volcano. Chimboraco a volcano.

The mountain of Tunguragua has diminiſhed in height ſince the earthquake of 1797. Bouguer aſſigns it 5589 yards, I found it but 5399, ſo that it muſt have loſt 190 yards; and indeed the people in the vicinity ſay, that they have ſeen its ſummit crumble away before their eyes. Tunguragua diminiſhed in height. Now 5399 yards.

During our ſtay at Riobancha, we accidentally made a very curious diſcovery. The ſtate of the province of Quito, previous to its conqueſt by the Inca Tupaynpangi, in 1470, is wholly unknown: but the king of the Indians, Leandro Zapla, who reſides at Lican, and has a mind extraordinarily cultivated for an Indian, poſſeſſes manuſcripts compoſed by one of his anceſtors, in the ſixteenth century, which contains the hiſtory of that period. They are written in the Parugay tongue, Indian manuſcripts of the 16th century.

tongue, which was formerly general in Quito, but is now loſt, having been ſupplanted by the Inca or Anichna. Fortunately another of Zapla's anceſtors amuſed himſelf by tranſlating theſe memoirs into Spaniſh. We have obtained from them valuable information, particularly in the memorable period of the eruption of Nevado del Atlas, which muſt have been the higheſt mountain in the world, loftier than Chimboraco, and called by the Indians *Capa-urcu*, or chief of mountains. Theſe manuſcripts, the traditions I collected at Parima, and the hieroglyphics I ſaw in the deſert of Caſiquiare, where ſcarcely a veſtige of a human being is now to be ſeen, added to what Clavigero has ſaid of the emigration of the Mexicans toward the ſouth, have ſuggeſted to me ideas reſpecting the origin of this people, which I ſhall purſue when I have leiſure.

Nevado del Atlas, once the higheſt mountain in the world.

Hieroglyphics.

I have likewiſe paid much attention to the ſtudy of the American languages, and found what Condamine has ſaid of their poverty to be extremely falſe. The Caribbee is rich, beautiful, energetic, and poliſhed: it is not deſtitute of expreſſions for abſtract ideas; and it has numerical terms ſufficient for any poſſible combination of figures. The Inca is particularly rich in delicacy and variety of expreſſion. The prieſts knew how to draw a meridian line, and obſerve the ſolſtices: they had reduced the lunar to a ſolar year by intercalations: and the ſavages even at Erevato, in the interior of Parima, believe the moon to be inhabited, and know, from the traditions of their anceſtors, that its light is derived from the ſun.

American languages not poor.

Caribee.

Inca.

Ancient ſcience.

At Monpox I made ſome very curious experiments on the reſpiration of the crocodile, having procured forty or fifty young ones. Inſtead of diminiſhing the quantity of the air in which it reſpires like other animals, the crocodile increaſes it. A crocodile placed in 1000 parts of atmoſpheric air, conſiſting of 274 oxigen, 15 carbonic acid, and 711 azot, increaſed it in an hour and forty-three minutes, by the addition of 124 parts. The carbonic acid had received an augmentation of 64 parts: the oxigen had been diminiſhed 167; but, as 46 are contained in the carbonic acid produced, the crocodile had appropriated to itſelf only 121 parts, a ſmall quantity conſidering the colour of its blood: and 227 parts of azot, or other gaſſes, on which acidifiable

Crocodile increaſes air by reſpiration.

Air 274 oxigen, 15 carbonic acid, 711 azot.

baſes

baſes had no action, were produced. For the analyſis I uſed lime water and nitrous gas, prepared with great care.

Large foſſil bones at a conſiderable height, and nearly from one extremity of America to the other.

Near Santa Fee, at an elevation of 2890 yards, are found an immenſe number of foſſil bones of elephants, both of the African ſpecies and of the carnivorous kind, diſcovered in North America. I have ſince received others from a part of the Andes, about the latitude of 2° from Quito, and from Chili: ſo that theſe animals muſt have exiſted from Patagonia to the Ohio.

VIII.

Method of meaſuring any Aliquot Part of an Inch by a Screw, which gives no ſuch Part in its Turn; and Obſervation on an Error of Edwards in placing the Eye Stop of reflecting Teleſcopes. In a Letter from Mr. J. C. HORNBLOWER.

To Mr. NICHOLSON.

DEAR SIR,

Conſtruction of a reticulated ſquare.

BEING on a viſit about fifteen years ago in the confines of the principality, (a region ſcience never yet explored) I was in want of a reticulated ſquare to be placed in the focus of the eye glaſs of an optical inſtrument, and by the diſappointment uſually attending jobs of this kind when done at a diſtance, I reſolved on accompliſhing it myſelf, though I foreſaw it would be a difficult undertaking, eſpecially as I am but an indifferent workman even with the beſt tools, and with ſuch as lay before me, I could not anticipate much pleaſure in my taſk.

A ſcrew was made by tapping.

The firſt thing which occurred to me was, the conſtruction of the old faſhioned micrometer, and I found a tap which promiſed well for the purpoſe; but unfortunately it had no determinate number of threads; but nevertheleſs I was reſolved to proceed, and by this tap I cut a pair of dies, and by them I cut a ſcrew on a piece of large braſs wire, which being more homogenous than ſteel in general, had a better chance of the accuracy my inſtrument required, however I ſet the nut in a jimbal, and on trying the ſcrew, I found it had 26,6 threads in an inch, and as I wanted tenths of an inch, I muſt neceſſarily provide means to correct the exceſs or defect in the ſcrew, which I did thus:

The

This was fitted up with a micrometer plate, and had a clip, by which the ſcrew could be held faſt while the plate was ſhifted.

The ſcrew had a collar, which I could draw together ſo as to hold it faſt, and another collar, on which I put the divided plate, and after that an index firmly fixed on the ſcrew. The plate alſo having liberty to turn on its collar, I could faſten it independent of the ſcrew, by having two thumb ſcrews oppoſite each other, bearing on the periphery of the plate, ſo that I could turn the ſcrew only, or turn the plate only, or turn them all together.

The ſcrew was turned, the quantity anſwering to 0.1 inch; then clipped faſt and the micrometer ſet to zero: the ſcrew was ſet free and moved as before; then clipped, and the micrometer ſet; the ſcrew then ſet free and moved, &c. &c.

The operation was as follows: Having laid the frame I had to divide on the board, to which this apparatus was connected, I brought the index to 0 on the plate, and made it (the plate) faſt, then turned the index two revolutions and ,66 of a revolution: This gave me a diviſion of ,1 inch. I then faſtened the ſcrew by bringing the collar to bind upon it; liberated the plate, and turned it until 0 on the diviſions came to the index. The plate was then faſtened, and the ſcrew ſet free and turned 2,66 revolutions as before. The ſcrew was then again faſtened and the plate ſet free, and 0 brought to the index as before, after which the plate was faſtened, and the ſcrew ſet free and turned, &c. With a little addition of apparatus to make the faſtenings and looſenings inſtantaneous; this method may be uſed to much advantage when a more elaborate or ſcientific inſtrument cannot be obtained.

I forgot in our converſation on the ſubject of reflecting teleſcopes, to bring to your mind a falſe calculation of Mr. Edwards's in his directions for making ſpecula for reflecting teleſcopes, publiſhed in the nautical almanack ſome years ſince.

Mr. Edwards aſſerts that the eye ſtop of a reflecting teleſcope ought to be in the focus of the eye glaſs.

Concerning the place of the eye ſtop, his words are "It is abſolutely neceſſary for perfect viſion that the eye ſhould be applied to a ſmall hole of a certain dimenſion, placed exactly in the focus of the ſingle eye-glaſs, if the eye-piece conſiſt of one glaſs only, or elſe in the compound focus of the glaſſes; if it is conſtructed with two, as is moſt commonly practiſed;" and afterwards ſays, "Let the diſtance of the eye-hole from the eye-glaſs, if it is a ſingle one, be put as near as can be attained by meaſure, equal to the focal diſtance of the eye-glaſs, &c."

But it ought not to be at the focus of parallel rays; but of rays from the vortex of the ſmaller ſpeculum.

Surely Mr. Edwards muſt have taken this on a bare ſuppoſition, without ever enquiring why. When we ſpeak of the focus of a lens, it is generally underſtood to mean the focus of parallel rays, but the focus of parallel rays does not determine

mine the place of the eye ſtop, but a focus, where the image of the ſmall ſpeculum is formed, and this will be more or leſs accordingly as the diſtance between the lens and the ſpeculum is leſs or more. For inſtance, take a lens of 1,5 inch focus, which would ſuit a ſpeculum of 9 inches focus, and the diſtance of the ſmall ſpeculum will be about 14 inches from the eye glaſs. Then $\frac{14 \times 1,5}{14-1,5} = 1,68$ will be the true diſtance of the eye ſtop, or in other words, the focus of rays proceeding from an object placed 14 inches beyond the lens, almoſt two inches farther than the ſolar focus.

I am,

DEAR SIR,

Your moſt obedient Servant,

J. C. HORNBLOWER.

IX.

Account of a new Apparatus conſtructed for the Purpoſe of meaſuring the Elaſtic Force, and regulating the Emiſſion of Steam from the Boiler in which it is generated. Communicated by the Inventor, Mr. ARTHUR WOOLF, *Engineer.*

Self acting and regulating ſteam valve.

PLATE XIV. exhibits a meaſured ſection of the ſelf-acting and regulating ſteam valve. A A repreſents the upper part of the boiler, having its mouth or neck cylindrical. and cloſed by a well-fitted, but eaſily moving valve plug B B C C, which is in fact a metallic tube, open at bottom and cloſed above, by a cap-piece B B, that by its chamfered rim or projecting part affords the accurate valve-cloſure when down. The interior parallel lines at D ſhew the place where a long perforation is made through the ſide of the cylindrical part of the valve plug from its cap, nearly down to the bottom; which perforation affords a paſſage for the ſteam, increaſing in magnitude as the elaſtic force cauſes the valve to riſe. E is the ſide paſſage for conveying the ſteam to its place of operation. F is the rod or tail of the valve paſſing through a ſtuffing box above, and attached by a chain to the ſector G, and by its means moving the lever that carries the ball H.

The

The above conſtitutes the whole of this ſimple and effectual contrivance, and its mode of operation ſcarcely needs to be deſcribed. As the ſteam becomes ſtronger it raiſes the valve, and eſcapes through D, and raiſes the weight H higher the more the preſſure within exceeds that of the working ſteam in the upper ſpace F E.

X.

Journey to the Summit of Mont-Perdu. By Cit. RAMOND *.

Sauſſure's travels in the Alps highly uſeful to geology.

THE many excellent obſervations made by the celebrated Sauſſure in the Alps, traverſing that grand aſſemblage of mountains in all directions, have contributed more effectually to the advancement of geology, than all the hypotheſes that have been formed.

Ramond's in the Pyrenees.

Cit. Ramond is rendering a ſimilar ſervice to the ſcience, by his repeated journies in the Pyrenees; and his adventurous reſearches will ſoon bring us acquainted with a great part of that chain, the ſtructure of which is ſo different from that of the Alps. In a work publiſhed two years ago, he deſcribed the baſis of Mont-Perdu: he had even approached its ſummit, and had obſerved that this mountain, the loftieſt of the Pyrenees, was calcareous, and contained ſhells and other organized bodies, in a foſſil ſtate, at an elevation of about 3600 metres.

Reached the ſummit of Mont Perdu.

In the journey he made in Auguſt 1802, he reached the ſummit of the mountain by paſſing the Col de Fanlo, or Niſcle.

Vertical ſtrata of carbonated lime.

In this road he conſtantly found ſtrata of compact carbonated lime in a poſition nearly vertical. They include ſtrata of calcareous ſandſtone, and theſe ſtrata ſometimes cover the ſalient angles of the vertical ſtrata, nearly in a horizontal direction.

Horizontal of calcareous ſandſtone.

This calcareous ſtone falls off ſpontaneouſly in little irregular fragments: on the ſlighteſt friction it emits a nauſeous fetid ſmell. Some of the ſtrata of this ſtone contain nodules of flint; others ſuch conſiderable maſſes of *camerines*, that the ſtone appears entirely compoſed of them.

Summit a fetid limeſtone.

The ſummit is formed of a fetid lime-ſtone, contaminated with quartz, and containing a little iron, and $\frac{1}{350}$ of carbon, without alumine. Cit. Ramond found no fragments of ſhells: but this ſtone being of a ſimilar

* Bulletin des Sciences.

kind

kind to the neighbouring ſtrata, in which they occur, he is inclined to believe, that he ſhould diſcover ſome on a more ſedulous reſearch. The elevation of this ſummit is the ſame as that of the Col du Géant in the Alps, or 3426 metres.

Its height 3727 yards.

From this, the loftieſt point of the Pyrenean chain, Cit. Ramond could more eaſily obſerve the general form of the whole. Looking toward France, the chain is broad, and formed of ſeveral parallel lines of mountains, in the midſt of which are ſeen the lines of granite and gneiſs, of which the peak of Bagneres is a part. Theſe are more diſtinguiſhable by their ſummits being rugged with aſperities, than for their elevation. Theſe lines imperceptibly diminiſh in height till they reach the plain, which is too far diſtant to be ſeen. To the ſouth the appearance is very different. The whole declines ſuddenly, and at once. It is a precipice of ten or eleven hundred metres, the bottom of which is the ſummit of the higheſt mountains of this part of Spain. Not one of them, however, has 2500 metres abſolute height, and they ſoon ſink into low rounded hills, beyond which is the vaſt proſpect of the plains of Arragon.

Parallel lines of mountains on the French ſide

Granite and gneiſs in the middle.

On the Spaniſh ſide a lofty precipice:

From the ſummit of Mont-Perdu, on the Spaniſh ſide, is ſeen a vaſt flat of limeſtone, the ſurface of which, from that elevation, appears almoſt ſmooth. This flat is interſected by four or five vaſt chaſms with perpendicular ſides, the angles and ſinuoſities of which correſpond to each other with aſtoniſhing exactneſs. Theſe broad and deep chaſms diverge from the baſe of the peak, and their bottoms are covered with thick woods. There is no way of entering them bnt at their mouths. Cit. Ramond proceeded by the way of Val de Broto, and entered that called by the natives Val d'Ordeſa. It is a deep valley, uninhabited, and bordered by ſteep walls about 896 metres high. Theſe you can aſcend only in few places, and with the greateſt difficulty. You then reach the flat. The ſtrata that form it, and in which theſe vaſt chaſms have been opened, are horizontal, or very little inclined. They conſiſt of red ſandſtone of ancient formation, pudding-ſtone, and compact limeſtone. All theſe ſtones are diſpoſed to break off in a direction perpendicular to their beds, and Cit. Ramond aſcribes this diſpoſition to the quartz they contain. He thinks that the chaſms, opened at firſt by ſome unknown cauſe, have been enlarged by the crumbling of their ſides in this manner.

Beneath this a flat of limeſtone,

interſected by vaſt chaſms.

Val d'Ordeſa.

The ſtrata red ſandſtone, pudding-ſtone, and limeſtone.

On

The peaks on the two ſides riſe in oppoſite directions.

On approaching the peaks that riſe from this flat, the ſtrata, which are of compact ſhelly limeſtone, riſe at an angle of 45°, but in a direction contrary to that of the ſtrata that form the ſmall peaks on the northern, or French ſide. Thus theſe ſtrata as they riſe diverge like the ſticks of an opened fan, the vertical ones conſtitute the ſummits; a remarkable arrangement, which Cit. Ramond aſcribes to a ſliding of the ſtrata, rather than a riſing up, properly ſo called, from a depreſſion of the other end.

Cit. Ramond has aſcertained the limits of permanent ſnow, and of vegetation, for this lofty part of the Pyrenean chain.

Limits of permanent ſnow, and of vegetation.

The ſnow terminates at 2440 metres. The laſt trees are Scotch firs, which reach 2150 metres. Next come the ſhrubs, of which the juniper is the higheſt. At 2760 metres are found the *ranunculus parnaſſiæ-folius*, the *ſaxifraga Groenlandica*, &c. then the *artemiſia rupeſtris* of Lamarck; and laſtly, round the very peak of Mont-Perdu. on rocks too ſloping to retain the ſnow, grow the *ceraſtium*, perhaps the *alpinum* of Linneus, and the roſe-flowering *arctia alpina*.

XI.

Notice of a Method of giving the Appearance of Cotton to Hemp or Flax. By Cit.* BERTHOLLET.

The author's former experiments.

WHILE I was engaged in the application of the oxigenated muriatic acid, to the art of bleaching, I made experiments on flax, and I inſerted this obſervation in the firſt volume of *Elemens de Tainture*, p. 258. "I have endeavoured to bleach flax completely, by the method I make uſe of with thread; but although its filaments may not loſe much of their ſolidity by this, they nevertheleſs acquire ſuch a tendency to ſeparate and divide themſelves, as renders them much more difficult to ſpin, and they form a thread of much leſs ſolidity."

Subſequent attempts.

Since that period different artiſts have employed themſelves, with various ſucceſs, in methods of obtaining from flax a matter analogous to cotton. A Swiſs, Cit. Clays has even formed an eſtabliſhment ſome time ago, in which this kind of preparation is executed.

* From Journal de L'Ecole Polytechnique, Tome IV. p. 319.

I know

I know not the proceſſes which have been heretofore employed, but I have ſucceeded, by means of the oxigenated muriatic acid, in obtaining a matter more beautiful than any of thoſe, the knowledge of which has reached me.

The very ſimple proceſs I am about to deſcribe, was executed in the laboratory of the Polytechnic School, by Cit. Gai-Luſſac, at that time one of it pupils.

Berthollet's proceſs.

The flax is cut into fragments about ſix centimetres long; it is covered with water in which it is left for three or four days; after this it is boiled in ſimple water; it is waſhed with care; it is lyed, it is put into oxigenated muriatic acid. Four immerſions in the oxigenated muriatic acid and four lyes are commonly ſufficient: the operation is finiſhed by immerſing it in a bath of water charged with one two-hundredth part of ſulphuric acid. On removing it from this tepid bath in which it has been left for near half an hour, it is waſhed with great care, and plunged into water charged with ſoap: it is then ſpread, without being wrung, on hurdles, where it is left to dry, without, however, ſuffering it to become too dry. All theſe operations, from the firſt immerſion to the drying, do not require more than five or ſix hours, when the proceſs is made with ſmall quantities.

Mechanical operations.

The flax thus prepared was then ſent to Cit. Molar, who was kind enough to undertake the mechanical operations: he firſt combed and then carded the bleached flax. He experienced ſome difficulties from the knots that were ſcattered through the flax, but this ſkilful mechanic ſoon overcame this inconvenience. I preſented to the claſs of phyſical and mathematical ſciences of the Inſtitute, on the 6th Prairial of the year 8, a ſample of the prepared materials, which was equal to cotton in its whiteneſs, and other apparent qualities; nevertheleſs, Cit. Molar found fault with the cottony matter for being too ſhort in the ſtaple.

The filaments are too ſhort.

Cit. Bawens alſo manufactured the cottony matter prepared in the laboratory of the ſchool, with the beautiful machines which he poſſeſſes at his manufactory at Chaillot. He found no difficulty in the execution, but he alſo found the filaments too ſhort, although he procured a very fine thread of conſiderable tenacity.

Propoſed remedy.

It is therefore the inconvenience of being reduced into ſhort filaments which requires to be corrected in the firſt preparation;

preparation; and I am of opinion one certain method of accompliſhing it, is not to complete the bleaching, but to ſtop at the third operation. If four are required for the thorough bleaching, it muſt then be finiſhed in the thread or in the ſtuff.

Directions for the bleaching.

In the operation of bleaching, too ſtrong lyes muſt be avoided, but they muſt be made uſe of in boiling. We are convinced that all the means which diminiſh the odour of the oxigenated muriatic acid weaken its action; hence it muſt be employed in a ſtate of purity, and we muſt not attempt to preſerve ourſelves from its odour, but by the conſtruction of the apparatus and the mode of application, objects which uſe has rendered eaſy: it muſt even be uſed in its concentrated ſtate, otherwiſe the operations require to be much increaſed.

The proceſs was finiſhed by immerſion in water charged with ſoap, which was not preſſed out, in order that the filaments might not adhere too much by drying, but yield eaſily the ſeparation which is to be performed by the card. But there is a probability that by preventing too much drying, the inconvenience experienced in the firſt trials would not take place, and that this immerſion might then be omitted.

The cottony matter is obtained equally good from fine flax or coarſe hemp.

It is remarkable that whether the fineſt flax or the coarſeſt hemp is made uſe of, the filaments obtained are of equal fineneſs and colour.

This indication will be guide enough to artiſts, well acquainted with chemical manipulations, in the operation of bleaching. But I have nothing to ſay on the mechanical diſpoſitions of carding and ſpinning, becauſe they were not executed by me.

Probable advantages to be obtained by the proceſs.

If I am not deceived, this application of a proceſs already old, offers many advantages, becauſe it may change the fabrication of thread, which, to this day, requires the ſpinning-wheel into that much leſs expenſive, which is executed by means of machinery; and it may convert a rough product of our agriculture, and even ſome of the refuſe, ſuch as that from rope-walks, into a ſubſtance valuable in the arts. This motive has induced me to inſert this notice in the Journal of an eſtabliſhment devoted to public utility, although it offers nothing new as a ſcientific ſubject.

XII. *Experimental*

XII.

Deſcription of a Machine now in actual and daily Uſe, for cleanſing Chimnies, without the Aſſiſtance of Climbing-boys, and with much greater Effect than is produced by that Method. Communicated by the Inventor, Mr. CHRISTOPHER SMART, *of Ordnance Wharf, Weſtminſter Bridge.* W. N.

Machine for cleanſing chimnies.

EVERY humane perſon muſt have beheld with pain and regret, the infant victims of a filthy and diſguſting operation, who are expoſed to daily ſuffering, and too often to permanent diſeaſe and decrepitude, without the hope of ſubſiſtence when grown to manhood. A remedy for the caſe of theſe unhappy and devoted children was long ago attempted by the amiable and benevolent Jonas Hanway; and within the laſt twelve months, another philanthrophiſt, James Hebdin, eſq; has actively exerted himſelf to form a ſociety for promoting and eſtabliſhing methods of cleaning chimnies more worthy of a great and civilized people, than one grounded on the miſery of the weak and the helpleſs. My gratulations, and thoſe of every good man, will form but a ſmall portion of that reward which the internal conſciouſneſs of the extenſive good he has done muſt afford. I ſhall therefore diſmiſs any farther conſideration of the perſonal merits of thoſe by whom the attention of mechanics has been directed to this object, or of the artiſts who have laboured to ſolve the problem offered to their ingenuity; and ſhall proceed to deſcribe Mr. Smart's machine. This apparatus has been approved upon trial by the ſociety lately eſtabliſhed, and has anſwered to the ſatisfaction of ſeveral well qualified employers, whoſe certificates I have ſeen, by bringing down more than the uſual quantity of ſoot, as well as by its efficacy in lofty or winding chimnies, and ſuch as are too narrow to be ſwept by children. It may eaſily be inferred, that it muſt be ſtill more advantageous in chimnies on fire, than the ſhocking proceſs of ſending up a child wrapped in rags to enter an actual place of combuſtion and ſuffocating vapour.

Plate XIII. *Fig.* 1. repreſents an apparatus of bruſhes, ſuppoſe four, which are fixed by hinges to a middle piece or bar, ſo

Machine for clearing chimnies.

ſo that they ſhall be capable either of hanging down, parallel to the bar, or of being opened and expanded, ſomewhat in the manner of an umbrella, until they ſtand out at right angles with the middle piece; in which ſituation they are retained by ſmall ſupporting bars, reſembling thoſe of the ſame well-known utenſil. *Fig.* 2. ſhews the bruſhes in their collapſed ſtate, with an appendage of tubes, by which the ſyſtem is thruſt up the chimney. A ſtrong cord is paſſed through a ſeries of theſe tubes, the lower mouth of every one of which is opened a little, in order to admit the upper ends of each in ſucceſſion. *Fig.* 5. ſhews the ſweeping man employed, thruſting the apparatus up the chimney; in which the ſet of connected tubes forms a piece, having enough of flexibility to accommodate itſelf to the chimney, and yet ſufficiently rigid to anſwer the purpoſe of carrying up the ſet of bruſhes. When theſe have paſſed through the chimney-pot, and given the uſual evidence of the work being to be performed from one end of the chimney to the other, the rope is drawn tight in order to ſet the tubes ſteadily together, and then ſecured by a thumb ſcrew ſeen in *Fig.* 2.—after which, the ſweeper begins to pull it downwards. The rim of the narrow opening of the chimney-pot cauſes the bruſhes to expand, and in this ſtate they are retained by the uſual ſpring-catch ſeen in *Fig.* 1. and by the ſimple and gradual deſcent, the chimney becomes effectually cleared of its ſoot.

Fig. 3. repreſents a curtain for defending the apartment againſt the ſoot. It is ſupported by a rod of metal, having a cork fixed or ſtuck in one of its ends, to afford a ſpringy and perfectly harmleſs bearing againſt the inſide lining of the chimney-piece, whether of marble or any other material, and the clamp which is ſeen towards the other end of the rod, has likewiſe a facing of cork, and is fixed at any diſtance, ſo as to afford the oppoſite bearing. The ſides of the curtain are ſecured by rods, *Fig.* 4. which can be lengthened or ſhortened by two parts ſliding together, as is ſeen in the meaſuring rule of ſhoemakers, or perhaps more familiarly, in thoſe ſliding pencils, which have now been ſeveral years in uſe.

I have been aſſured that the cleanlineſs, decency, and quiet operation of this engine, are by no means among the ſmalleſt of its recommendations. I have not yet had an occaſion of trying it in my own houſe, but ſhall certainly do it

on

on the firſt opportunity, and ſhall then either confirm or modify in a future notice, as my own obſervation ſhall direct, theſe particulars of information, which upon good grounds I have thought myſelf juſtified in now laying before my readers.

W. N.

XIII.

Experimental Eſſays on the Conſtitution of mixed Gaſes; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expanſion of Gaſes by Heat. By JOHN DALTON *.

The diſcovery of general laws is of high value in the advancement of ſcience.

THE progreſs of philoſophical knowledge is advanced by the diſcovery of new and important facts; but much more when thoſe facts lead to the eſtabliſhment of *general* laws. It is of importance to underſtand that the deſcent of falling bodies is the ſame every where on the ſurface of the earth; but from that and ſome other particular facts to infer the law of gravitation, or that all matter attracts with a force decreaſing as the ſquare of the diſtance, is a much higher attainment in ſcience. In the train of experiments lately engaging my attention ſome new facts have been aſcertained, which with others, ſeem to authorize the deduction of general laws, and ſuch as will have influence in various departments of natural philoſophy and chemiſtry.

* Theſe intereſting treatiſes were read before the literary and philoſophical Society at Mancheſter, in October, 1801, and are publiſhed in the fifth volume of their memoirs. The firſt, on mixed gaſes, was communicated in the ſame month, in a ſomewhat different form, by the author to our Journal, and publiſhed in the quarto ſeries, vol. V. p. 241.—and, a further communication from him on the ſame ſubject appeared in vol. III. p. 267 of our preſent ſeries. I have not, therefore, reprinted that eſſay of the preſent collection. The laſt eſſay in the title, viz. on the expanſion of gaſes, is inſerted in the laſt mentioned volume, p. 130.—Conſequently though the title and introduction refer to the whole four; yet the preſent article contains only what was wanting to complete the readers poſſeſſion of this valuable maſs of experimental knowledge; that is to ſay, the eſſays upon ſteam and upon evaporation.

Statement of certain laws previous to their fundamental experiments.

As the detail of experiments will be beſt underſtood and their application ſeen, if the laws of principles alluded to be kept in view, it may be proper here to ſtate them; though it muſt not be underſtood that they were proceeded upon hypothetically in the direction of thoſe experiments. On the contrary, the firſt law, which is as a mirror in which all the experiments are beſt viewed, was *laſt* detected, and after all the particular facts had been previouſly aſcertained.

1. Mixed elaſtic fluids do not repel each other.

1. When two elaſtic fluids, denoted by *A* and *B*, are mixed together, there is no mutual repulſion amongſt their particles; that is, the particles of *A* do not repel thoſe of *B*, as they do one another. Conſequently, the preſſure or whole weight upon any one particle ariſes ſolely from thoſe of its own kind.

2. The ſteam of any liquid, at any given number of degrees from its boiling point, acts the ſame as that of any other liquid at the ſame number of degrees, the ſame way from its boiling point.

2. The force of ſteam from all liquids is the ſame, at equal diſtances above or below the ſeveral temperatures at which they boil in the open air: and that force is the ſame under any preſſure of an other elaſtic fluid as it is in vacuo. Thus, the force of *aqueous* vapour of 212° is equal to 30 inches of mercury; at 30° below, or 182°, it is of half that force; and at 40° above, or 252°, it is of double the force; ſo likewiſe the vapour from ſulphuric ether which boils at 102°, then ſupporting 30 inches of mercury, at 30° below that temperature it has half the force, and at 40° above it, double the force: and ſo in other liquids. Moreover, the force of aqueous vapour of 60° is nearly equal to half inch of mercury, when admitted into a torricellian vacuum; and water of the ſame temperature, confined with perfectly dry air, increaſes the elaſticity to juſt the ſame amount.

3. Evaporation at any temperature is as the force of the ſteam.

3. The quantity of any liquid evaporated in the open air is directly as the force of ſteam from ſuch liquid at its temperature, all other circumſtances being the ſame.

4. All elaſtic fluids expand equally by heat.

4. All elaſtic fluids expand the ſame quantity by heat: and this expanſion is very nearly in the ſame equable way as that of mercury; at leaſt from 32° to 212°.—It ſeems probable the expanſion of each particle of the ſame fluid, or its ſphere of influence, is directly as the quantity of heat combined with it; and conſequently the expanſion of the fluid as the cube of the temperature, reckoned from the point of total privation.

Having

Having now ſtated the chief principles which ſeem to be eſtabliſhed from the following ſeries of facts and obſervations, I ſhall proceed to treat of them under the ſeveral heads *.

ESSAY II.

On the Force of Steam or Vapour from Water and various other Liquids, both in a Vacuum and in Air.

SECTION I.—*On Vapour in Vacuo.*

THE term *ſteam* or *vapour* is equally applied to thoſe elaſtic fluids which, by cold and preſſure of certain known degrees, are reduced wholly or in part into a liquid ſtate. Such are the elaſtic fluids ariſing from water, alkohol, ether, ammonia, mercury, &c. Other elaſtic fluids that cannot be reduced, or rather that have not yet been reduced, into a liquid ſtate by the united agency of thoſe two powers, are commonly denominated *gaſes*. There can ſcarcely be a doubt entertained reſpecting the reducibility of all elaſtic fluids of whatever kind into liquids; and we ought not to deſpair of effecting it in low temperatures and by ſtrong preſſure exerted upon the unmixed gaſes. However uneſſential the diſtinction between the gaſes and vapours may be in a chemical ſenſe, their *mechanical* action is very different. By increaſing the quantity of any gas in a given ſpace the force of it is proportionally increaſed; but increaſing the quantity of any liquid in a given ſpace does not at all affect the force of the vapour ariſing from it. On the other hand, by increaſing the temperature of any gas a proportionate increaſe of elaſticity enſues; but when the temperature of a liquid is increaſed, the force of vapour from it is increaſed with amazing rapidity, the increments of elaſticity forming a kind of geometrical progreſſion, to the arithmetical increments of heat.—Thus, the ratio of the elaſtic force of atmoſpheric air of 32° to that at 212°, is nearly as 5 : 7; but the ratio of the force of aqueous vapour proceeding from water of 32° and 212°, is as 1 : 150 nearly.

Steam or vapour defined. It is an elaſtic fluid capable of becoming liquid by cold and preſſure. Gaſes not ſo.

Remarkable difference between the expanſion of ſteam and of gas by heat: the former being prodigiouſly greater.

The object of the preſent eſſay is to determine the utmoſt force that certain vapours, as that from water, can exert at different temperatures. The importance hitherto attached to this enquiry has ariſen chiefly from the conſideration of ſteam

Object of the preſent eſſay.

* Here followed the eſſay I. on mixed gaſes. W. N.

as a mechanical agent; and this has directed the attention more eſpecially to high temperatures. But it will appear from what follows that the progreſs of philoſophy is more immediately intereſted in accurate obſervations on the force of ſteam in low temperatures. Different authors have publiſhed accounts of their experiments on the force of ſteam: I have on a former occaſion (Meteorological Eſſays, page 134) given a table of forces for every 10° from 80° to 212°. The author of the article "Steam," in the Encyclopedia Britannica, has done the ſame from 32° to 280°: and M. Betancourt, in the "Memoirs des ſcavans etrangeres" for 1790, ſee Hutton's Math. Diction. page 755) has given tables on the ſubject, both for vapour from water and ſpirit of wine, alſo from 32° to 280°. But theſe two authors, having aſſumed the force of vapour from water of 32° to be nothing, are eſſentially wrong at that point and in all the lower parts of the ſcale; and in the higher part, or that above 212°, they determine the force too much: owing as I apprehend to a quantity of air, which being diſengaged from the water by heat and mixing with the ſteam, increaſes the elaſticity.—In a queſtion of ſuch moment it ſeemed therefore deſirable to obtain greater accuracy.

Reference to authors reſpecting ſteam.

Encyl. Britt.

Betancourt.

My method is this: I take a barometer tube perfectly dry, and fill it with mercury juſt boiled, marking the place where it is ſtationary; then having graduated the tube into inches and tenths by means of a file, I pour a little water (or any other liquid the ſubject of experiment) into it, ſo as to moiſten the whole inſide; after this I again pour in mercury, and, carefully inverting the tube, exclude all air: the barometer by ſtanding ſome time exhibits a portion of water, &c. of $\frac{1}{5}$ or $\frac{1}{10}$ of an inch upon the top of the mercurial column; becauſe being lighter it aſcends by the ſide of the tube; which may now be inclined and the mercury will riſe to the top manifeſting a perfect vacuum from air. I next take a cylindrical glaſs tube open at both ends, of 2 inches diameter and 14 inches in length; to each end of which a cork is adapted, perforated in the middle ſo as to admit the barometer tube to be puſhed through and to be held faſt by them; the upper cork is fixed two or three inches below the top of the tube and is half cut away ſo as to admit water, &c. to paſs by; its ſervice being merely to keep the tube ſteady. Things being thus circumſtanced

The author's method. A minute portion of the fluid is put into the upper ſpace of a barometer. Heat is applied by the external contact of water. The fall of the mercury ſhews the effect.

ſtanced, water of any temperature may be poured into the wide tube, and thus made to ſurround the upper part or vacuum of the barometer, and the effect of temperature in the production of vapour within can be obſerved from the depreſſion of the mercurial column. In this way I have had water as high as 155° ſurrounding the vacuum: but as the higher temperatures might endanger a glaſs apparatus; inſtead of it I uſed the following:—

The veſſel containing the external water was of tin for temperatures near boiling; and a ſyphon barometer was uſed to ſhew the depreſſion.

Having procured a tin tube of four inches in diameter and two feet long, with a circular plate of the ſame ſoldered to one end having a round tube in the center like the tube of a reflecting teleſcope, I got another ſmaller tube of the ſame length ſoldered into the larger, ſo as to be in the axis or centre of it: the ſmall tube was open at both ends, and on this conſtruction water could be poured into the large veſſel to fill it, whilſt the central tube was expoſed to its temperature. Into this central tube I could inſert the upper half of a ſyphon barometer, and fix it by a cork, the top of the narrow tube alſo being corked; thus the effect of any temperature under 212° could be aſcertained, the depreſſion of the mercurial column being known by the aſcent in the exterior leg of the ſyphon.

Another method by obſerving the boiling point in the air-pump, and the ſtation of the barometer gage.

The force of vapour from water between 80° and 212° may alſo be determined by means of an air-pump; and the reſults exactly agree with thoſe determined as above. Take a Florence flaſk half filled with hot water, into which inſert the bulb of a thermometer; then cover the whole with a receiver on one of the pump-plates, and place a barometer gage on the other: the air being ſlowly exhauſted, mark both the thermometer and barometer at the moment ebullition commences, and the height of the barometer gage will denote the force of vapour from water of the obſerved temperature. This method may alſo be uſed for other liquids. It may be proper to obſerve the various thermometers uſed in theſe experiments were duly adjuſted to a good ſtandard one.

Hence the force of ſteam up to 212° was bad.

After repeated experiments by all theſe methods, and a careful compariſon of the reſults, I was enabled to digeſt the following table of the force of ſteam from water in all the temperatures from 32° to 212°.

Two important enquiries ſtill remained, the firſt to determine the force of ſteam from water above 212° and below

32°; the ſecond, to determine the comparative forces of vapour from other liquids. Theſe enquiries ſeemed independent of each other; notwithſtanding which I found them in reality connected.

Examination of the progreſſion of the force of vapour.

Upon examination of the numbers in the table, within the limits juſt mentioned, there appears ſomething like a geometrical progreſſion in the forces of vapour; the ratio, however, inſtead of being conſtant, is a gradually diminiſhing one: thus the

Force at	32°	=	,200	inch.
			17. 50	} Ratios.
	122	=	3. 500	
			8. 57	}
	212	=	30. 000	

If we divide theſe ratios, according to obſervation, they will ſtand thus:

Force at	32°	=	,200	inch.
			4. 550	} Ratios.
	77	=	,910	
			3. 846	
	122	=	3. 500	
			3. 214	
	167	=	11. 250	
			2. 666	}
	212	=	30. 000	

If we divide theſe again, they become,

Force at	32°	=	,200	inch.
			2. 17	} Ratios.
	$54\frac{1}{2}$	=	,435	
			2. 09	
	77	=	,910	
			2. 00	
	$99\frac{1}{2}$	=	1. 820	
			1. 92	
	122	=	3. 500	
			1. 84	
	$144\frac{1}{2}$	=	6. 450	
			1. 75	
	167	=	11. 250	
			1. 67	
	$189\frac{1}{2}$	=	18. 800	
			1. 59	}
	212	=	30. 000	

By

By another diviſion we obtain the ratios for every $11\frac{1}{4}°$ of temperature from 32° to 212°, as under; Examination of the progreſſion of the force of vapour.

Force at	32° =	,200 inch	
			1. 485
	$43\frac{1}{4}$ =	,297	
			1. 465
	$54\frac{1}{2}$ =	,435	
			1. 45
	$65\frac{3}{4}$ =	,630	
			1. 44
	77 =	,910	
			1. 43
	$88\frac{1}{4}$ =	1. 290	
			1. 41
	$99\frac{1}{2}$ =	1. 820	
			1. 40
	$110\frac{3}{4}$ =	2. 540	
			1. 38
	122 =	3. 500	Ratios.
			1. 36
	$133\frac{1}{4}$ =	4. 760	
			1. 35
	$144\frac{1}{2}$ =	6. 450	
			1. 33
	$155\frac{3}{4}$ =	8. 550	
			1. 32
	167 =	11. 250	
			1. 30
	$178\frac{1}{4}$ =	14. 600	
			1. 29
	$189\frac{1}{2}$ =	18. 800	
			1. 27
	$200\frac{3}{4}$ =	24. 000	
			1. 25
	212 =	30. 000	

Thus it appears that a ratio having a uniform decreaſe nearly takes place; and we may therefore extend the table of forces at both extremes, without the aid of experiment, to a conſiderable diſtance. Thus aſſuming the ratios for each interval of a $11°\frac{1}{4}$ below 32° to be, 1.500, 1.515, 1.530, 1.545, &c. and for each interval above 212° to be 1.235, 1.220, 1.205, 1.190, 1.175, 1.160, 1.145, 1.130, &c. we can extend the table many intervals of temperature, and determine all the intermediate degrees by interpolation This method may be relied upon as a near approximation; The ratio of augmentation in the force of vapour is not as the degrees, but gradually leſs.

however it does not ſuperſede the expediency of determination by experiment; though that is much more difficult above 212°, and below 32°, than in the intermediate degrees; becauſe it is difficult to procure a ſteady heat above 212°; and below 32° the variation of force becomes ſo ſmall as to elude minute diſcrimination. It will appear from what follows that the extenſion of the table by this method above 212° is in all probability accurate, or very nearly ſo, for 100° or more.

TABLE

Table of the force of aqueous vapour or ſteam.

Of the Force of Vapour from Water in every temperature from that of the congelation of Mercury, or 40° below zero of Fahrenheit, to 325°.

Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.
-40°	,013	25°	,156	54°	,429
-30	,020	26	,162	55	,443
-20	,030	27	,168	56	,458
-10	,043	28	,174	57	,474
—	—	29	,180	58	,490
0	,064	30	,186	59	,507
1	,066	31	,193	60	,524
2	,068	—	—	61	,542
3	,071	32	,200	62	,560
4	,074	33	,207	63	,578
5	,076	34	,214	64	,597
6	,079	35	,221	65	,616
7	,082	36	,229	66	,635
8	,085	37	,237	67	,655
9	,087	38	,245	68	,676
10	,090	39	,254	69	,698
11	,093	40	,263	70	,721
12	,096	41	,273	71	,745
13	,100	42	,283	72	,770
14	,104	43	,294	73	,796
15	,108	44	,305	74	,823
16	,112	45	,316	75	,851
17	,116	46	,328	76	,880
18	,120	47	,339	77	,910
19	,124	48	,351	78	,940
20	,129	49	,365	79	,971
21	,134	50	,375	80	1. 00
22	,139	51	,388	81	1. 04
23	,144	52	,401	82	1. 07
24	,150	53	,415	83	1. 10

Tem-

Table continued.

Table of the force of aqueous vapor or ſteam.

Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of VaP. in inches of Mercury.
84°	1. 14	128°	4. 11	172°	12. 73
85	1. 17	129	4. 22	173	13. 02
86	1. 21	130	4. 34	174	13. 32
87	1. 24	131	4. 47	175	13. 62
88	1. 28	132	4. 60	176	13. 92
89	1. 32	133	4. 73	177	14. 22
90	1. 36	134	4. 86	178	14. 52
91	1. 40	135	5. 00	179	14. 83
92	1. 44	136	5. 14	180	15. 15
93	1. 48	137	5. 29	181	15. 50
94	1. 53	138	5. 44	182	15. 86
95	1. 58	139	5. 59	183	16. 23
96	1. 63	140	5. 74	184	16. 61
97	1. 68	141	5. 90	185	17. 00
98	1. 74	142	6. 05	186	17. 40
99	1. 80	143	6. 21	187	17. 80
100	1. 86	144	6. 37	188	18. 20
101	1. 92	145	6. 53	189	18. 60
102	1. 98	146	6. 70	190	19. 00
103	2. 04	147	6. 87	191	19. 42
104	2. 11	148	7. 05	192	19. 86
105	2. 18	149	7. 23	193	20. 32
106	2. 25	150	7. 42	194	20. 77
107	2. 32	151	7. 61	195	21. 22
108	2. 39	152	7. 81	196	21. 68
109	2. 46	153	8. 01	197	22. 13
110	2. 53	154	8. 20	198	22. 69
111.	2. 60	155	8. 40	199	23. 16
112	2. 68	156	8. 60	200	23. 64
113	2. 76	157	8. 81	201	24. 12
114	2. 84	158	9. 02	202	24. 61
115	2. 92	159	9. 24	203	25. 10
116	3. 00	160	9. 46	204	25. 61
117	3. 08	161	9. 68	205	26. 13
118	3. 16	162	9. 91	206	26. 66
119	3. 25	163	10. 15	207	27. 20
120	3. 33	164	10. 41	208	27. 74
121	3. 42	165	10. 68	209	28. 29
122	3. 50	166	10. 96	210	28. 84
123	3. 59	167	11. 25	211	29. 41
124	3. 69	168	11. 54	212	30. 00
125	3. 79	169	11. 83		
126	3. 89	170	12. 13	213	30. 60
127	4. 00	171	12. 43	214	31. 21

Tem-

Table of the force of aqueous vapor or ſteam.

Table continued.

Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.
215°	31. 83	252°	60. 05	289°	98. 96
216	32. 46	253	61. 00	290	100. 12
217	33. 09	254	61. 92	291	101. 28
218	33. 72	255	62. 85	292	102. 45
219	34. 35	256	63. 76	293	103. 63
220	34. 99	257	64. 82	294	104. 80
221	35. 63	258	65. 78	295	105. 97
222	36. 25	259	66. 75	296	107. 14
223	36. 88	260	67. 73	297	108. 31
224	37. 53	261	68. 72	298	109. 48
225	38. 20	262	69. 72	299	110. 64
226	38. 89	263	70. 73	300	111. 81
227	39. 59	264	71. 74	301	112. 98
228	40. 30	265	72. 76	302	114. 15
229	41. 02	266	73. 77	303	115. 32
230	41. 75	267	74. 79	304	116. 50
231	42. 49	268	75. 80	305	117. 68
232	43. 24	269	76. 82	306	118. 86
233	44. 00	270	77. 85	307	120. 03
234	44. 78	271	78. 89	308	121. 20
235	45. 58	272	79. 94	309	122. 37
236	46. 39	273	80. 98	310	123. 53
237	47. 20	274	82. 01	311	124. 69
238	48. 02	275	83. 13	312	125. 85
239	48. 84	276	84. 35	313	127. 00
240	49. 67	277	85. 47	314	128. 15
241	50. 50	278	86. 50	315	129. 29
242	51. 34	279	87. 63	316	130. 43
243	52. 18	280	88. 75	317	131. 57
244	53. 03	281	89. 87	318	132. 72
245	53. 88	282	90. 99	319	133. 86
246	54. 68	283	92. 11	320	135. 00
247	55. 54	284	93. 23	321	136. 14
248	56. 42	285	94. 35	322	137. 28
249	57. 31	286	95. 48	323	138. 42
250	58. 21	287	96. 64	324	139. 56
251	59. 12	288	97. 80	325	140. 70

On Vapour from Ether, &c.

Vapor from ether and from other liquids follow a general law in their force.

We come now to the conſideration of vapour from other liquids. Some liquids are known to be more evaporable than water; as liquid ammonia, ether, ſpirit of wine, &c. others leſs; as, quickſilver, ſulphuric acid, liquid muriate of lime,

lime, ſolution of potaſh, &c. and it appears that the force of vapour from each in a vacuum is proportionate to its evaporability. M. Betancourt maintains that the force of vapour from ſpirit of wine is in a conſtant ratio to that from water at all temperatures; namely, as 7 to 3 nearly. My firſt experiments with ſpirits of wine led me to adopt this concluſion, and naturally ſuggeſted that the force of vapour from any other liquid would bear a conſtant ratio to that of water. The principle, however, is not true, either with regard to ſpirit of wine or any other liquid. Experiments made upon ſix different liquids agree in eſtabliſhing this as a general law; namely, *that the variation of the force of vapour from all liquids is the ſame for the ſame variation of temperature, reckoning from vapour of any given force:* thus aſſuming a force equal to thirty inches of mercury as the ſtandard, it being the force of vapour from any liquid boiling in the open air, we find *aqueous* vapour loſes half its force by a diminution of 30° degrees of temperature; ſo does the vapour of any other liquid loſe half its force by diminiſhing its temperature thirty degrees below that in which it boils; and the like for any other increment or decrement of heat. This being the caſe, it becomes unneceſſary to give diſtinct tables of the force of vapour from different liquids, as one and the ſame table is ſufficient for all. But it will be proper to relate the experiments on which this concluſion reſts.

The law enunciated.

Experiment on Sulphuric Ether.

The ether I uſed boiled in the open air at 102.—I filled a barometer tube with mercury, moiſtened by agitation in ether. After a few minutes a portion of ether roſe to the top of the mercurial column, and the height of the column became ſtationary. When the whole had acquired the temperature of the air in the room, 62°, the mercury ſtood at 17.00 inches, the barometer at the ſame time being 29.75. Hence the force of vapour from ether at 62 is equal to 12.75 inches of mercury, which accords with the force of aqueous vapour at 172°, temperatures which are 40° from the reſpective boiling points of the liquids. By ſubſequent obſervations I found the forces of the vapour from ether in all the different temperatures from 33° to 102° exactly correſponded with the forces of aqueous vapour of the like range

Experiments with ether; in the barometer, below ebullition.

range, namely from 142° to 212°: the vapour from ether depreſſes the mercury about ſix inches in the temperature of 32°.

above ebullition

Finding that ether *below* the point of ebullition agreed with water below the ſaid point, I naturally concluded that ether *above* the point would give the ſame force of vapour as water above it; and in this I was not diſappointed; for, upon trial it appeared that what I had inferred only from analogical reaſoning reſpecting the force of aqueous vapour above the boiling point, actually happened with that from ether above the ſaid point. And ether is a much better ſubject for experiment in this caſe than water, becauſe it does not require ſo high a temperature.

with the ſyphon barometer.

I took a barometer tube of 45 inches in length, and having ſealed it hermetically at one end, bent it into a ſyphon ſhape, making the legs parallel, the one that was cloſe being nine inches long, and the other 36. Then conveyed two or three drops of ether to the end of the cloſed leg, and filled the reſt of the tube with mercury, except about 10 inches at the open end. This done, I immerſed the whole of the ſhort leg containing the ether into a tall glaſs containing hot water; the ether thus expoſed to a heat above the temperature at which it boils, produced a vapour more powerful than the atmoſphere, ſo as to overcome its preſſure and raiſe a column of mercury beſides, of greater or leſs length according to the temperature of the water. When the water was at 147° the vapour raiſed a column of 35 inches of mercury, when the atmoſpheric preſſure was 29.75: ſo that vapour from ether of 147° is equivalent to a preſſure of 64.75 inches of mercury; agreeing with the force of aqueous vapour of 257°, according to the preceding eſtimation: in both caſes the temperatures are 45° above the reſpective points of ebullition. In all the temperatures betwixt 102° and 147° the forces of ethereal vapour correſponded with thoſe of aqueous vapour, as per table, betwixt 212° and 257°. I could not reaſonably doubt of the equality continuing in higher temperatures; but the force increaſes ſo faſt with the increaſe of heat, that one cannot extend the experiments much farther without tubes of very inconvenient lengths. Being deſirous however to determine the

Experiments in which the reaction of included air was uſed as the meaſure of force.

force

force of the ethereal vapour experimentally up as high as 212°, I contrived to effect it as follows:—Took a ſyphon tube ſuch as deſcribed above, only not quite ſo long, and filled it in the manner above mentioned, with ether and mercury, leaving about ten inches at the top of the tube vacant; then having graduated that part into equal portions of capacity, and dried it from ether, I drew out the end of the tube to a capillary bore, cooled it again ſo as to ſuffer the internal atmoſpheric air to be of the proper denſity, and ſuddenly ſealed the tube hermetically, thus incloſing air of a known force in the graduated portion of the tube. Then, putting that part of the tube containing ether into boiling water, vapour was formed which forced the mercurial column upwards and condenſed the confined air, till at length an equilibrium took place. In this way I found 8.25 parts of atmoſpheric air of the force 29.5 were condenſed into 2.00, at the ſame time a perpendicular column of 16 inches of mercury in addition preſſed upon the vapour. Now the force of elaſtic fluids being inverſely as the ſpace, we have 2.00 : 29.5 : : 8.25 : 121.67 inches = the force of the air within; to which adding 16 inches, we obtain 137.67 = the whole force ſuſtained by the vapour, meaſured in inches of mercury. The force of aqueous vapour, at the ſame diſtance beyond the boiling point, or 322°, is equal to 137.28, per table. Thus it appears that in every part of the ſcale on which experiments have been made, the ſame law of force is obſervable with the vapour of ether as of water.

Experiments on Spirit of Wine.

Force of vapor of ſpirit of wine.

By boiling a ſmall portion of the ſpirit I uſed (about one cubic inch) in a phial, the thermometer ſtood at 179° at the commencement; but by continuing the ebullition it acquired a greater heat. The reaſon is, the moſt evaporable part of the ſpirit flies off during the proceſs of heating, and the reſt being a weaker compound, requires a ſtronger heat. The true point of ebullition, I believe, was nearly 175°.—The force of the vapour from this ſpirit at the temperature of 212°, I found both by an open ſyphon tube and one hermetically ſealed with atmoſpheric air upon the mercurial column, as with ether, to be equal to 58½ inches of mercury.

This

This rather exceeds the force of aqueous vapour at an equal diſtance from the boiling point; but it is no more than may be attributed to unavoidable little errors in ſuch experiments. In a barometer tube the ſpirituous vapour at 60°, over the mercury, depreſſes the column about 1.4 or 1.5 inches; which is ſomething leſs than the due proportion; one cauſe of this may be the evaporability of ſpirits, which in operating on ſmall quantities, quickly diſſipates part of their ſtrength.

Experiments on Liquid Ammonia.

Force of vapor of liquid ammonia,

Liquid ammonia or volatile alkali, the ſpecific gravity of which was .9474, boiled near 140°; in the barometer a ſmall quantity depreſſed the mercury 4.3 inches in the temperature of 60°. In higher temperatures it did not produce a proportional depreſſion; becauſe the moſt volatile part of the compound, expanding in the vacuum of the barometer, leaves the reſt more watery, and conſequently its vapour muſt be weaker; eſpecially when the portion uſed is confined to a drop or two.

Muriate of Lime.

of muriate of lime.

Put a portion of liquid muriate of lime over the column of mercury in a barometer. The boiling point of the muriate was found by experiment to be 230°. At 55° the depreſſion was ,22 of an inch:

at 65°—.30
— 70°—.40
— 95°—.90

all which nearly agree with the forces of aqueous vapour 18° below the reſpective temperatures.

Mercury and Sulphuric Acid.

Forces of the vapour of mercury and ſulphuric acid.

Mercury boils by my thermometer at 660°, and ſulphuric acid of the ſpecific gravity 1.83, boils at 590°. It is very difficult to determine the preciſe force of vapour from theſe liquids in any temperature under 212°; becauſe at ſuch great diſtance from the boiling point the vapour is ſo weak as to be in effect almoſt imperceptible, Following the general law, the vapours of theſe fluids ought to be of the force .1, mercury at 460°. and ſulphuric acid at 390°.—Col. Roi makes the expanſion of 30 inches mercury by 180o of heat = .5969 or .5651; and in a barometer the expanſion in the

ſame

ſame circumſtances is .5117 ; the differences are .0852 and .0534 which ſhould meaſure the effective force of mercurial vapour of 212°, nearly. This is in all probability too much ; as it is next to impoſſible to free any liquid entirely from air ; and if *any* air enter the vacuum, it unites its force to that of the mercurial vapour.

That the force of vapour from ſulphuric acid, in low temperatures, is exceedingly ſmall, will appear from the enſuing ſection.

SECTION II.

On Vapour in Air.

Effect of the expanſion of vapour in air. The experiments made by a ſmall tube, ſtopped by a moveable plug of mercury.

The experiments under this head were made with manometers, or ſtraight tubes of different lengths, hermetically ſealed at one end, of $\frac{1}{15}$ inch internal diameter, and their capacities divided into equal portions. A drop or two of the liquid, the ſubject of experiment, was conveyed to the bottom or ſealed end of the tube; the internal ſurface was then dried by a wire and thread, and atmoſpheric, (or any other air) was admitted into the tube, upon which a column of mercury was ſuſpended of $\frac{1}{10}$ of an inch, or of 30 inches, leſs or more, according to the nature of the experiment. By immerſing the end of the manometer, containing the air thus circumſtanced, into a tall glaſs veſſel containing water of any temperature, the effect of the vapour in expanding the air could be perceived. It was firſt indeed neceſſary to determine the increaſe air unaffected by any liquid (except mercury) would obtain by increaſe of temperature: that was done, as will be particularly ſhewn in the next eſſay.* The expanſion of all elaſtic fluids, it ſeems probable, is alike or nearly ſo, in like circumſtances; 1000 parts of any elaſtic fluid expands nearly in a uniform manner into 1370 or 1380 parts by 180° of heat.

General law of expanſion of air and vapour together. The ſpace at any temperature is directly as the preſſure and inverſely as the preſſure leſs the force of the vapour.

It will be unneceſſary to repeat in detail the numerous experiments made on the various liquids in all temperatures from 32° to 212°; as the reſults of all agree in one general rule or principle, which is this: let 1 repreſent the ſpace occupied by any kind of air of a given temperature and free from moiſture; p = the given preſſure upon it, in inches of mercury ; f = the force

* Philoſophical Journal, Vol. III. page 130.

force of vapour from any liquid in that temperature, in vacuo; then, the liquid being admitted to the air, an expanſion enſues, and the ſpace occupied by the air becomes immediately, or in a ſhort time $= 1 + \frac{f}{p-f}$; or, which is the ſame thing, $= \frac{p}{p-f}$.

Thus in water for inſtance:

Let $p = 30$ inches,

$f = 15$ inches, to the given temp. 180°.

Then, $\frac{p}{p-f} = \frac{30}{30\text{-}15} = 2$, for the ſpace; or the air becomes of twice the bulk.

If the temperature be 203°, $f = 25$, and the ſpace becomes ſix times as large as at firſt.

If $p = 60$ inches

$f = 30$ inches to the given temperature 212°; then the ſpace $= \frac{60}{60-30} = 2$; or water under the preſſure of 60 inches of mercury, and at the temperature of 212°, produces vapour which juſt doubles the volume of air.

If ether be the inſtance: let the temperature be equal 70°; then $f = 15$; and ſuppoſe $p = 30$; in this caſe the colume of air is doubled; that is, ether of 70° being admitted to any portion of air, doubles its bulk.

The expanſion of hydrogenous gas and atmoſpheric air by the vapour of water is the ſame for every temperature.

Sulphuric acid does not expand atmoſpheric air to any ſenſible amount by the heat of boiling water.

The theory of theſe facts is evident upon the principles laid down in the former eſſay: for inſtance; let it be required to explain the experiment with water of 212° under a preſſure of 60 inches. Here the air was condenſed into the ſpace 1 by the preſſure of 60 inches; but being expoſed to water of 212°, a vapour aroſe from it equal in force to 30 inches; the air therefore expanded till its force alſo beeame = to 30 inches, which was effected by doubling its volume: then the vapour preſſing with 30 inches force and the air alſo with 30 inches force, the two together ſupport the preſſure of 60 inches and the equilibrium continues. In ſhort, in all caſes the vapour ariſes to a certain

certain force, according to temperature, and the air adjuſts the equilibrium, by expanding or contracting as may be required.

The notion of a chemical affinity ſubſiſting between the gaſes and vapours of different kinds, cannot at all be reconciled to theſe phenomena. To ſuppoſe that all the different gaſes have the ſame affinity for water might indeed be admitted if we could not explain the phenomena without it; but to go further, and ſuppoſe that water combines with every gas to the ſame amount as its vapour in vacuo; or in other words, that the elaſticity of the compound ſhould be exactly the ſame as if the two were ſeparate, is certainly going far to ſerve an hypotheſis.

Theſe facts do not agree with the notion of chemical affinity between gas and vapour.

Beſides, we muſt on this ground ſuppoſe that all the gaſes have the ſame force of affinity for any given vapour; a ſuppoſition that cannot be admitted as having any analogy to other eſtabliſhed laws of chemical affinity.

(To be continued.)

XIV.

Deſcription of the Portable Furnace conſtructed by Dr. Black, and ſince improved. In a Letter from Mr. Accum.

To Mr. NICHOLSON.

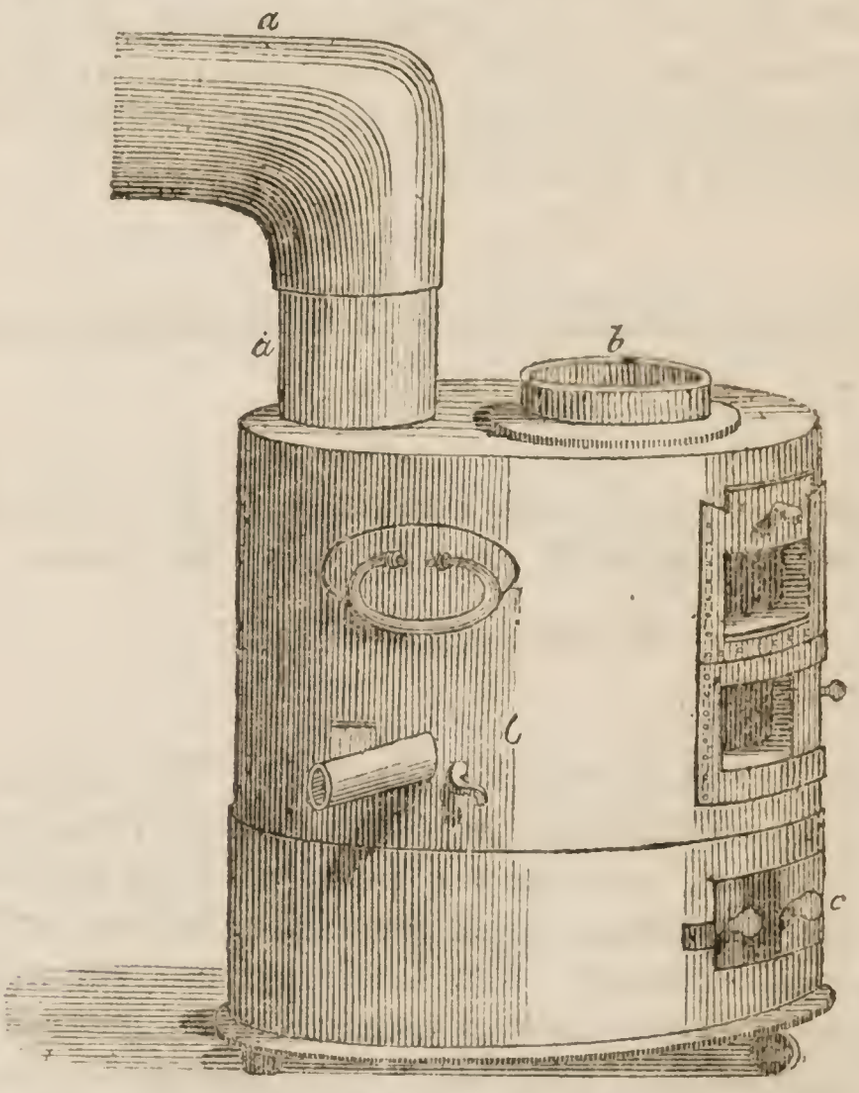
Dear Sir,

IN my Syſtem of Practical Chemiſtry, Vol. II. p. 357, I have given a Deſcription and Drawing of a Portable Univerſal Furnace, which in the practice of my profeſſion I found the beſt furnace for all chemical operations whatever which require like aid. The number of furnaces which I have cauſed to be made for different philoſophers of that kind, and the uſeful hints which I have received from different quarters, have materially improved it, that I flatter myſelf, whether a deſcription of this furnace would not be acceptable to your readers, particularly to thoſe who have no acceſs to the laboratory of the operative chemiſt; for thoſe who are familiar with practical chemiſtry will readily allow, that a furnace capable of producing a very low and very intenſe heat is one of the moſt requiſite and moſt indiſpenſible inſtruments of all the apparatus of chemiſtry. The

Deſcription of the portable furnace in chemical operations.

Deſcription of the portable furnace in chemical operations.

great advantage of this furnace (*which was firſt invented by Dr. Black, and improved by others*) above all others I am acquainted with, conſiſts in conſuming as little fuel as poſſible, in producing quickly, if required, a very intenſe heat—in regulating expeditiouſly, and at pleaſure, its intenſity—in applying it as directly, and as fully as poſſible, to the ſubſtances upon which it is intended to act—and moreover in enabling the operator to perform his operations in the cloſet, or in any other place, without the riſk of endangering the conflagration of the ſurrounding objects, which were not meant to be expoſed to the action of heat.



This portable univerſal furnace is made of ſtrong wrought iron plates. It is lined with bricks, bedded in fire-proof loam. Its height without the chimney *a a* is two feet. The inner diameter of the cylindrical fire-place meaſures ten inches. The body of the furnace is elliptical; in its upper part a circular hole is cut, for receiving an iron ſand-pot *b* which may occaſionally be removed and exchanged for an iron plate. In the front of the furnace there are three openings over each other, furniſhed with ſliding doors, and fitted with

with ſtoppers made of crucible ware. The lower opening *c*, is the aſh-pit of the furnace; it is compoſed of two regiſter plates, ſliding backwards and forwards in grooves, in order to diminiſh, or enlarge the opening for regulating the heat, by admitting or excluding air at pleaſure. In the ſide of the furnace a hole is cut, furniſhed with a ſtopper and door, for paſſing a tube through the fire-place of the furnace; an expedient very neceſſary for a variety of chemical proceſſes, ſuch as exhibiting the decompoſition of water, alcohol, oils, &c. for the preparation of phoſphuret of lime, for paſſing gaſes over ignited bodies, &c. In either of the openings in front of the furnace, a muffle may be placed for performing the proceſs of cupellation of gold, ſilver, &c. or, the neck of a retort (placed on a ſtand in the body of the furnace) may be paſſed through it, for diſtillation by the naked fire; for procuring gaſes which require a high degree of heat, &c. If the iron ſand-pot *b* be removed, and a circular plate properly lined with fire-clay be placed in its room, the furnace becomes converted into a wind-furnace; the fuel is then to be introduced through either of the openings in front. The iron plate at the top has a hole in the centre, furniſhed with a ſtopper, to enable the operator to inſpect his proceſs at pleaſure. If the iron-pot be placed inverted on the opening of the furnace, it forms a dome, and it then becomes a reverberating furnace. The iron-pot when filled with ſand, or water, placed in its proper ſituation, ſerves as a ſand or water-bath, for the proceſſes of diſtillation by means of glaſs retorts, for evaporations, ſublimations, digeſtions, &c. Coake and charcoal are the beſt fuel, this mixture burns without ſmoke, and gives a ſtrong uniform and permanent heat; charcoal and common coal, or coal only, does likewiſe very well. The elbow of the chimney *a* may be directed into that of the fire-place of any apartment.

Deſcription of the portable furnace in chemical operation.

The furnace is furniſhed with caſtors, and may therefore be eaſily moved, according to the convenience of the operator. I am, SIR,

Your moſt obedient ſervant,

FREDERICK ACCUM.

Old Compton-Street, Soho,
Nov. 21, 1803.

XV.

Observations on the Structure of the Tongue; illustrated by Cases in which a Portion of that Organ has been removed by Ligature. *By* EVERARD HOME, *Esq. F. R. S.**

Introduction.

PHYSIOLOGICAL inquiries have ever been considered as deserving the attention of this learned Society; and, whenever medical practitioners, in the treatment of diseases, have met with any circumstance, which threw light upon the natural structure or actions of any of the organs of the human body, or those of other animals, their communications have met with a favourable reception.

Importance of a safe means of removing part of the tongue.

The following observations derive their real importance from offering a safe and effectual means of removing a portion of the tongue, when that organ has taken on a diseased action, the cure of which is not within the reach of medicine; and, as the tongue, like many other glandular structures, is liable to be affected by cancer, it becomes of no small importance that the fact should be generally known. In a physiological view, they tend to show, that the internal structure of the tongue is not of that delicate and sensible nature which, from its being the organ of taste, we should be led to imagine.

Its structure

The tongue is made up of fasciculi of muscular fibres, with an intermediate substance met with in no other part of the body, and a vast number of small glands; it has large nerves passing through it; and the tip possesses great sensibility, fitting it for the purpose of taste,

Whether the sense of taste is confined entirely to the point of the tongue, and the other parts are made up of muscles fitted for giving it motion; or whether the whole tongue is to be considered as the organ, and the soft matter which pervades its substance, and fills the interstices between the fasciculi of muscular fibres, is to be considered as connected with sensation, has not, I believe, been ascertained.

supposed to be very delicate.

The tongue, throughout its substance, has always been considered by physiologists as a very delicate organ; and it was believed, that any injury committed upon it would not

* Philos. Transact. 1803, p. 205.

only produce great local irritation, but alſo affect, in a violent degree, the general ſyſtem of the body. This was my own opinion, till I met with the following caſe, the circumſtances of which induced me to ſee this organ in a different point of view.

Accident of the tongue very much bitten, and rendered inſenſible,

A gentleman by an accident which it is unneceſſary to deſcribe, had his tongue bitten with great violence. The immediate effect of the injury was great local pain; but it was not attended with much ſwelling of the tongue itſelf, nor any other ſymptom, except that the point of the tongue entirely loſt its ſenſibility, which deprived it of the power of taſte: whatever ſubſtance the patient eat was equally inſipid. This alarmed him very much, and induced him to ſtate to me the circumſtances of his caſe, and requeſt my opinion. I examined the tongue a fortnight after the accident. It had the natural appearance, but the tip was completely inſenſible, and was like a piece of board in his mouth, rendering the act of eating a very unpleaſant operation. I ſaw him three months afterwards, and it was ſtill in nearly the ſame ſtate.

From this caſe it appears, that the tongue itſelf is not particularly irritable; but the nerves paſſing through its ſubſtance to ſupply the tip, which forms the organ of taſte, are very readily deprived of their natural action; this probably ariſes from their being ſofter in texture than nerves in general, and in that reſpect, reſembling thoſe belonging to the other organs of ſenſe.

without inflammation or irritation to produce ſpaſms.

There was another circumſtance in this caſe which very particularly ſtruck my attention, *viz.* that a bruize upon the nerves of the tongue, ſufficient to deprive them of the power of communicating ſenſation, was productive of no inflammation or irritation in the nervous trunk, ſo as to induce ſpaſms, which too commonly occur from injuries to the nerves belonging to voluntary muſcles. I am therefore led to believe, that the nerves ſupplying an organ of ſenſe, are not ſo liable to ſuch effects as thoſe which belong to the other parts of the body.

Caſe of fungous excreſcence of the tongue.

The ſmall degree of miſchief which was produced, and the readineſs with which the nerves had their communication completely cut off, were to me new facts, and encouraged me, in

in the following caſe of fungous excreſcence from the tongue, which bled ſo profuſely as at times to endanger the patient's life, and never allowed him to arrive at a ſtate of tolerable health, to attempt removing the part by ligature.

which when removed was followed by violent hæmorrhage.

John Weymouth, eight years of age, was admitted into St. George's hoſpital, on the 24th of December, 1800, on account of a fungous excreſcence on the right ſide of the anterior part of the tongue, which extended nearly from the outer edge to the middle line at the tip. It appeared, from the account of his relations, that the origin of this fungus exiſted at his birth, and had been increaſing ever ſince. He had been a year and a half under the care of the late Mr. Cruikſhank, who had removed the excreſcence by ligature round its baſe; but, when the ligature dropped off, a violent hæmorrage took place, and the excreſcence gradually returned. Attempts were made to deſtroy it by cauſtic; but hæmorrhage always followed the ſeparation of the ſloughs; ſo that, after ten trials, this mode was found ineffectual. It was alſo removed by the knife, ten different times, but always returned.

From this hiſtory I was led to believe, that the only mode of removing the diſeaſe was taking out the portion of the tongue upon which it grew. This was a caſe in which I felt myſelf warranted in making an attempt out of the common line of practice, to give the patient a chance of recovery; and, from the preceding caſe, having found that preſſure on one part of the tongue produced no bad conſequences on the other parts, I was led to remove the excreſcence in the following manner.

The portion of tongue removed by ligature.

On the 28th of December, I made the boy hold out his tongue, and paſſed a crooked needle, armed with a double ligature, directly through its ſubſtance, immediately beyond the excreſcence. The needle was brought out below, leaving the ligatures; one of theſe was tied very tight before the excreſcence, the other equally ſo beyond it, ſo that a ſegment of the tongue was confined between theſe two ligatures, in which the circulation was completely ſtopped. The tongue was thin in its ſubſtance; and the boy complained of little pain during the operation. Thirty drops of laudanum were given to him immediately after it, and he was put to bed. He fell aſleep, continued to doſe the greater part of the day, and was ſo eaſy the next day as to require no particular attention.

On

On the fifth day from the operation, the portion of tongue came away with the ligatures, leaving a ſloughy ſurface, which was thrown off on the 11th day, and was ſucceeded by a ſimilar ſlough; this ſeparated on the 15th day. The excavation after this gradually filled up; and, on the 20th day, it was completely cicatrized, leaving only a ſmall fiſſure on that ſide of the tongue.

(To be concluded in our next.)

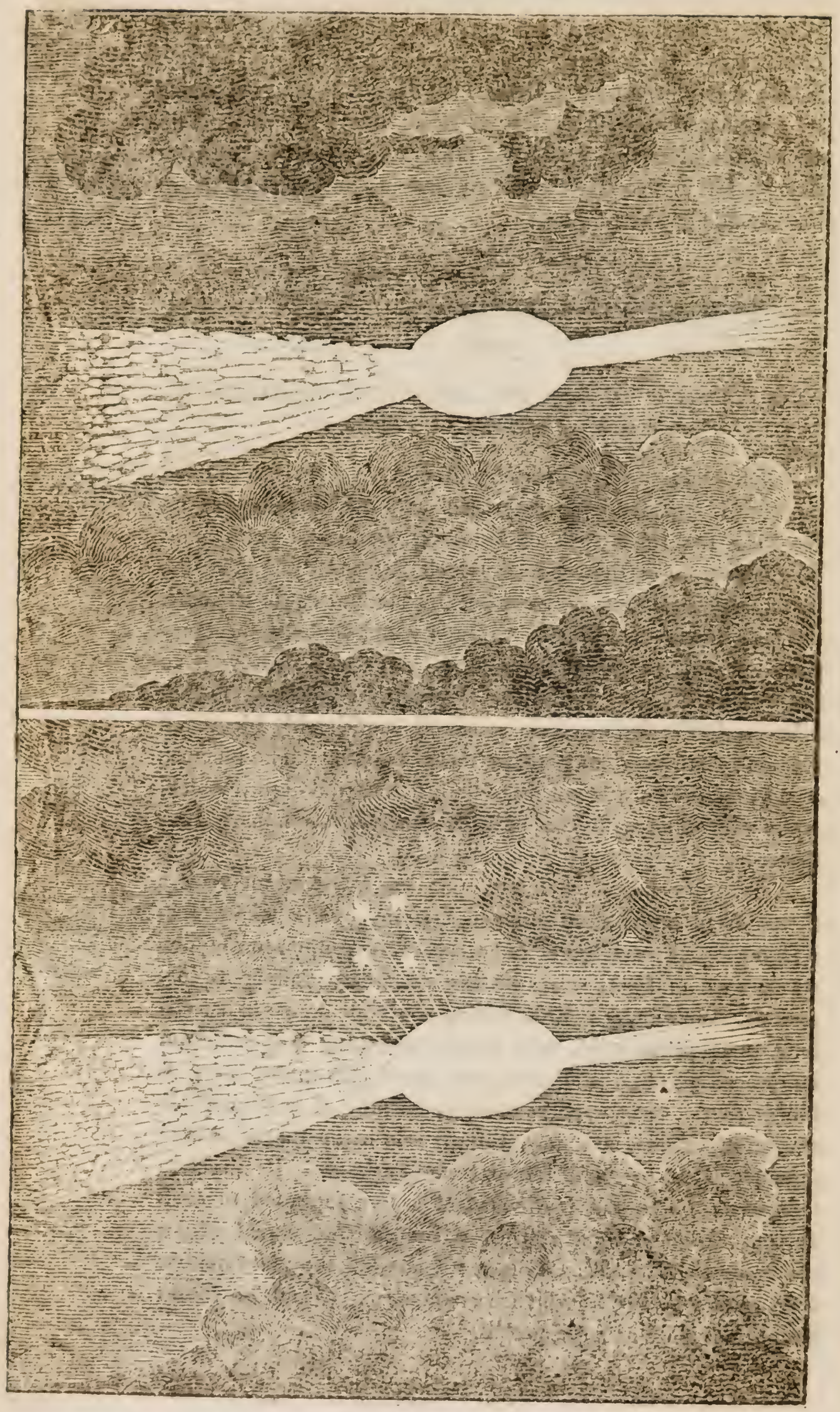
XVI.

Some Account of the large fiery Meteor which appeared on the ſixth of laſt Month (November.)

Account of a fiery meteor.

ON Sunday evening at half paſt eight, on the ſixth of laſt month, I was ſuddenly ſurprized with an illumination reſembling that of a flaſh of lightning, but more permanent. The windows of the room in which I was ſitting face the ſouth, and were not cloſed either by the ſhutters or curtains, but only by venetian blinds; through which ſome of the company aſſerted that they ſaw a large globe of fire moving to the weſtward. My back was towards the window, ſo that I ſaw only the light which appeared conſiderably blue, and ſeemed to laſt two, or perhaps three ſeconds. The blueneſs in all probability was not more than that of day-light, which, when contraſted with the light of candles, has a lively blue tinge.

A ſcientific friend of mine, who has favoured me with a ſketch from which the annexed drawings were taken, was walking up Princes Street Soho, and turned upon the ſudden appearance of light, when he ſaw the meteor paſſing rapidly over St. Ann's church yard, having the appearance of an oblong or elliptical ſolid, with a ſhort radiating eruption from its preceding part, and numerous ſparks diverging from its hinder part. He compares it to the burning of a combuſtible matter in oxigen, and ſaw it burſt into many ſparks, which inſtantly went out and left extreme darkneſs. Its direction ſeemed to be to the ſouthward of a line ſuppoſed to croſs Princes Street at right angles, which eſtimate would give a courſe about W. S. W. This gentleman ſaw the great meteor of Auguſt 18, 1783, which was then round, and he thinks the preſent quite as large as that.



Another friend informs me, that he ſaw it from a ſtation in St. James's Park, near the Queen's Houſe, riſing above the horizon, in the eaſt towards Weſtminſter-abbey, and that it paſſed over St. James's Park, and part of the Green Park, where it was loſt behind a cloud. He thinks it remained in ſight for a much longer time than two or three ſeconds, and that it did not move in a ſtraight line. The courſe by his obſervation would be about W. N. W. Account of a fiery meteor.

Another perſon who ſaw it burſt, ſpeaks of the portions falling down like the ſparks of a rocket.

That it paſſed as moſt large fiery meteors ſeem to do, in the ſuperior part of our atmoſphere is probable from the general facts. A gentleman on Hampſtead Heath, beheld the country ſuddenly illuminated, and clearly ſaw Harrow Steeple, which is eight miles diſtant in a ſtrait line, and it was alſo ſeen from Dartford in Kent, until it became obſcured by a cloud; but I had no further particulars reſpecting it.

Much difficulty muſt ariſe in eſtimating either the courſe, direction, or elevation of meteors of this kind, which appear when totally unexpected, and are gone before the mind can enter into any courſe of reaſoning or eſtimate. We are fully occupied with the impreſſive ſenſation they produce, and have ſcarcely any other means of obtaining a conjecture reſpecting poſitions and altitude, but by repeating our obſervations on the ſpot. On this ſubject the reader may conſult an excellent paper by Sir Charles Blagden in the LXXIV. volume of the Philoſophical Tranſactions.

The lower ſketch ſhews the manner of exploſion into ſmaller pieces.

XVII.

A First Memoir on coloured Shadows *. *By Cit.* J. H. Hassenfratz.

Shadows are generally but erroneously counted black.

We are accuſtomed to conſider as black, the ſhadow formed by an opaque body, which intercepts the light falling on a white paſteboard, although in reality, nothing is more difficult to obtain than black ſhadows, becauſe it appears, that to procure a black ſhadow, the light muſt abſolutely be a point, and no ſort of reflected light muſt reach the illuminated ſurface.

Phenomena of ſhadows.

On a ſurface illuminated by a light which has magnitude, the ſhadow is always accompanied by a coloured penumbra. Whenever the illuminating body is at a greater diſtance than a metre from the body enlightened, and that which intercepts the light, is nearly five decimetres from this body, the penumbra and ſhadow have diſtinct colours, which are in a great meaſure dependent upon the nature of the combuſtible body which yields the light.

The blue ſhadow at ſun-riſe and ſun-ſet, is not different from the general laws of ſhadows.

Since the period when Leonardo da Vinci noticed the blue ſhadow which is perceived in the morning at ſun-riſe, and in the evening at ſun-ſet, this ſhadow has been attended to, and naturaliſts have endeavoured to explain its production, as forming a phenomenon peculiar and diſtinct from thoſe of other ſhadows. Nevertheleſs, this ſhadow has no particularity; all the ſhadows which are examined are coloured, even thoſe produced by the noon-tide ſun of a clear ſummer's day, which are commonly called *black ſhadows*. The coloured ſhadows are produced by two or more diſtinct lights or by the ſeparated parts of the ſame light which act differently.

Variety of coloured ſhadows.

Nothing is more diverſified than the colours of ſhadows; on noticing them with attention, we remark among them all the priſmatic colours: we diſtinguiſh red, orange, yellow, green, blue, violet ſhadows, more or leſs mixed with black.

The ſubject is to be purſued beyond this Memoir.

The object of this firſt Memoir, is to make known the great variety of coloured ſhadows which may be diſtinguiſhed

* From Journal de L'Ecole Polytechnique, Tom. IV. p. 272.

and

and which we have obſerved, and alſo the circumſtances which give riſe to them. Other memoirs which we purpoſe to preſent after this, if the inſtitute ſhall think the ſubject worthy of its attention, will contain the ſeries of obſervations we have made to aſcertain the cauſes which produce the coloration of ſhadows in a great number of caſes.

We ſhall divide this Memoir into three parts: the firſt will have for its object the colour of the ſhadows produced by the reunion of the light of the atmoſphere, and that of the ſun, or of the light of the atmoſphere and an artificial light, but in thoſe circumſtances wherein only a ſingle ſhadow is perceptible: the ſecond will contain a deſcription of the coloured ſhadows produced by the light of the atmoſphere combined with reflected lights, and ſometimes with the direct light of the ſun: finally, the third will elucidate the coloured ſhadows formed on a body illuminated by two lights, natural and artificial. Diviſion of the ſubject.

Part the Firſt.

If, near a white ſurface enlightened by the ſun and the light of the atmoſphere, a black body is placed which intercepts the ſolar rays, there will be ſeen on the plane a ſhadow which varies from a greeniſh blue to a violet black, connecting through the blue and the violet. The colour of the ſhadow depends on the ſtate of the atmoſphere, the latitude of the place, the meridional and northern declination of the ſun, and the time elapſed between its riſing and its paſſage to the meridian, and from the latter to its ſetting. Obſervations of the nature of coloured ſhadows,

When the ſky is clear, the colour of the ſhadow at ſun-riſe, at Paris, varies between blue with a ſlight tinge of green to violet blue. The firſt days of Nivoſe, the ſhadow is greeniſh-blue; the firſt days of Germinal, blue; the firſt days of Meſſidor, indigo with a violet tinge; the firſt days of Vendemiaire, the ſhadow becomes again blue to return to a greeniſh blue the firſt of Nivoſe. as produced by change of ſeaſon,

If on a clear day, when the ſun is on the equator, the variation of the colour of the ſhadow, if noticed from the inſtant of ſun-riſe to its paſſage over the meridian, it will be obſerved that this colour is blue at ſun-riſe; that at each elevation of the luminary above the horizon, the blue changes; that it becomes indigo, violet; that in the end it blackens, and that when the ſun is on the meridian, the ſhadow is of a blackiſh violet. as produced by the meridional height of the ſun,

Every

as produced by the meridian ſun at different periods of the year,

Every day from the firſt of Nivoſe to the firſt of Meſſidor, the ſolar ſhadow offers different tints, at the moment when this luminary paſſes the meridian. The firſt days of Nivoſe, the ſhadow is violet, a little blackiſh; it increaſes in blackneſs daily to the firſt Meſſidor; at this period, the ſhadow is violet-black.

as produced by difference of latitude.

If the coloured ſhadow of the riſing ſun is obſerved on a clear day, at the ſame period, and in different latitudes, it will be ſeen to vary from violet-blue to green, going from the equator to the pole.

The coloured ſhadow obſerved at the beginning of Nivoſe, at ſun-riſe, from Meſſina to Skalhot in Iceland, is, at Meſſina, light indigo; at Vienna and at Paris, blue, with a ſlight green tinge; at London, Berlin, Copenhagen, Edinburgh, a more diſtinct green tinge; at Peterſburg, a little more green; finally at Drontheim and at Skalholt, a greeniſh tint.

At the ſame period, at noon, the ſhadow varies, between Meſſina and Skalholt, from black ſlightly tinged with violet to violet.

Reſult of the obſervations.

The compariſon of the colour of the ſhadow of the ſun, with its ſituation in the different places where it is obſerved, naturally leads to this firſt concluſion, that it is different in the ratio of the intenſity of its light, compared with that of the atmoſphere, in fact, the riſing ſun, on the firſt of Nivoſe, having a feeble light in compariſon with that of the atmoſphere, the ſhadow is a greeniſh blue; as it riſes above the horizon, the intenſity of its light increaſes, and the ſhadow becomes blue, indigo, violet: finally, when the ſun is on the meridian, its light has acquired its greateſt intenſity, and the ſhadow blackens, preſerving nevertheleſs a violet tinge.

Comparing in the ſame manner the colour of the ſhadow obſerved each day of the year at Paris, as well as that obſerved in each latitude on the ſame day, it will be ſeen, that it changes from green to violet-black, according to the intenſity of the light acquired by the ſun; and when in winter the ſun, being but little elevated above the horizon, appears red, from the feeble light which penetrates the light miſts exiſting in the air, the ſolar ſhadow is green, ſometimes a fine graſs green.

Experiments with an artificial light which is varied,

To ſatisfy ourſelves whether the colour of the ſhadow depended on the relation of the light compared to that of the atmoſphere, we placed the light of a lamp near a white ſurface, illuminated ſolely by the light of the atmoſphere; when this

lamp

lamp was five decimetres from the enlightened plane, the ſhadow cauſed by an opaque black body was blue. On bringing the lamp nearer the colour of the ſhadow changed ſucceſſively; from blue it changed to indigo, from indigo to violet, and the violet blackened gradually. When the lamp was very cloſe, the ſhadow was of a violet black colour, exactly ſimilar to that produced by the light of the ſun on a clear ſummer's day.

This experiment ſucceeds very well on days when the ſun is hid by clouds, all bodies are then illuminated by the light of the ſky.

A ſimilar reſult may be obtained by the inverſe method, that is to ſay, by illuminating the ſurface with an artificial light of conſtant intenſity, and ſucceſſively increaſing the intenſity of the light of the ſky. with an artificial light which is fixed.

If in a dull morning, before the appearance of twilight, a white ſurface is illuminated by the light of a lamp placed at five decimetres diſtant from the enlightened ſurface, the ſhadow of an opaque black body placed at a ſmall diſtance from the ſurface, is black very ſlightly violaceous. As ſoon as the twilight appears, the tint changes, the intenſity of the violet increaſes. As the day brightens, the violet of the ſhadow effaces the black tint; at length the ſhadow becomes violet, indigo, and is blue when the light of the day is completely developed.

Repeated experiments with a taper or a candle, have given the ſame reſults.

It follows, from the obſervations detailed, that every ſhadow, formed on a body illuminated, at the ſame time, by the light of the atmoſphere and the direct light of the ſun; or by the light of the ſky and an artificial light, ſuch as a lamp, a taper, a candle, is coloured in all caſes wherein the light of the ſun, or the artificial light is intercepted by an opaque black body; and that the colour of the ſhadow varies from green to violet black, in the ratio of the intenſity of the light of the ſun, or the artificial light, compared with that of the atmoſphere. But on what depends the colour of the atmoſphere? what cauſes that variation of colour in the ratio of the comparative intenſity? This is what we ſhall examine in another Memoir. General reſult.

(To be concluded in our next.)

SCIENTIFIC

SCIENTIFIC NEWS.

Obſervations on St. John's Wort. By Cit. BAUNACH.*

The juice of St. John's wort affords a red colour, and alſo a yellow.

ST. JOHN's wort, *hypericum perforatum*, is a reſinous plant, the tops and flowers of which contain a juice ſoluble in water, alcohol, and vinegar. With the former two it gives a blood-red colour, with the laſt a fine bright crimſon. When combined with [mineral?] acids or metallic ſolutions, it affords a yellow colour, which proves, that it contains two colouring matters, one, the red, more ſoluble than the other.

Red moſt ſoluble.

Mode of dyeing with it.

To dye linen, woollen, ſilk, or cotton yellow, it is ſufficient to put them into a bath, the water of which is duly impregnated with the juice of this plant, with a certain quantity of a mordant. The beſt mordant for this colour is alum combined with a ſuitable portion of potaſh. The ſtuff muſt be left in the bath ſome time; for the durability of the colour, and the ſhade produced, depend chiefly on the time of continuance in the bath, the quantity of the mordant, and the degree of heat employed. When but little mordant is uſed, the tint is a bright yellow; by increaſing it the colour is made to incline to green; and on adding a ſolution of tin in nitro-muriatic acid, it aſſumes roſe, cherry, and crimſon hues, all with a fine luſtre. Alum alone does not anſwer well, the addition of potaſh being eſſential. This decompoſes the alum, precipitates its earth, diſſolves a conſiderable portion of it, and this alcaline ſalt with an earthy baſe becomes the true mordant in the proceſs; the more becauſe the colouring principle reſides in a ſubſtance almoſt purely reſinous.

Various tints produced by it.

Alum and potaſh form the proper mordant.

Stains paper yellow,

and dyes leather.

The juice of St. John's wort, united with the mordant here mentioned gives a fine yellow colour to paper; and as it produces the ſame effect on leather, it may be employed with advantage for dying ſheep and other white ſkins.

Contains tannin.

The plant contains a conſiderable quantity of tannin, as I have been convinced by means of the ſolution of glue, and other experiments made for this purpoſe.

Sulphate of iron converts it into a concrete reſin.

On dropping a little ſolution of ſulphate of iron into the juice of St. John's wort, a blackiſh brown precipitate is formed, which has the property of abſorbing oxygen, becoming at length inſoluble in water, and aſſuming the characters of a concrete reſin.

* From the *Annales de Chimie*.

Having

Having diſtilled a certain quantity of the plant with water, the product had a powerful and agreeable ſmell, but I could not diſcover the leaſt trace of eſſential oil on it. Affords an agreeable water, but no eſſential oil.

The juice of St. John's wort does not diſſolve either in expreſſed or in eſſential oils, but it unites very well with reſins. For this purpoſe, the juice of the plant muſt be dried; which may be done very conveniently by expreſſing it into earthen plates, and placing theſe in an oven ſome time after the bread is drawn; it muſt then be powdered, and will readily combine with turpentine by rubbing them together in a braſs mortar warmed. This reſin, thus ſaturated with the juice, may be mixed with oils, either eſſential or expreſſed; and on combining it with oil of olives, the oil of St. John's wort of the ſhops may be formed, which, thus prepared, poſſeſſes evident virtues. If it be incorporated with linſeed oil, and a ſmall portion of oil of turpentine be added, a fine red varniſh is produced, which may be advantageouſly employed for coating articles of furniture made of wood. The juice does not diſſolve in oils, but unites with reſins. Mode of combining them. Oil of St. John's wort. Makes a fine red varniſh for wood.

Mr. RICHARD KNIGHT, who is well known to the philoſophical world for the very complete Magazine of Chemical Apparatus of all kinds he has for ſeveral years paſt eſtabliſhed in Foſter-Lane, has favoured me with a letter, in which he very ſatisfactorily ſhews that the inſtrument in *Plate* X. of our laſt number, was not invented by Mr. Accum, but by Mr. W. H. Pepys, about three years ago, and has ever ſince been an article on ſale in the catalogue of his warehouſe. The title to Mr. Accum's paper was written by myſelf, as almoſt all the titles are; and I was led to call him the inventor from the tenor of his paper. Immediately after the publication of laſt number, and before I had received any letter from Mr. Knight, Mr. Accum obſerved to me, that he is not the inventor, and that he firſt ſaw the inſtrument deſcribed in a German paper. At the ſame time therefore that I have the pleaſure to give Mr. Pepys the undiſputed right to a contrivance which, in point of utility and convenience, is of conſiderable value, I do not ſee any moral error that needs correction.

ACCOUNT

ACCOUNT OF NEW BOOKS.

The Edinburgh New Diſpenſatory; containing, I. *The Elements of Pharmaceutical Chemiſtry:* II. *The Materia Medica; or, the Natural, Pharmaceutical and Medical Hiſtory of the different Subſtances employed in Medicine:* III. *The Pharmaceutical Preparations and Compoſitions; including complete and accurate Tranſlations of the* 8vo. *Edition of the London Pharmacopœia, publiſhed in* 1791; *Dublin Pharmacopœia, publiſhed in* 1794; *and of the New Edition of the Edinburgh Pharmacopœia, publiſhed in* 1803. *Illuſtrated and explained in the Language, and according to the Principles of Modern Chemiſtry. With many new and uſeful Tables, and ſeveral Copper-plates, explaining the new Syſtem of Chemical Characters, and repreſenting the moſt uſeful Pharmaceutical Apparatus. By* ANDREW DUNCAN, *Jun. M. D. Fellow of the Royal College of Phyſicians, and Royal Society of Edinburgh, and Aſſociate of the Linnæan Society of London.* 8vo. 720 *Pages, and* 6 *Plates. Edinburgh,* 1803.

Duncan's Edinburgh New Diſpenſatory.

THE copious title page of this work informs the reader what he has to expect in this new edition of a ſtock book, the excellent foundation of which was laid by Dr. Lewis in 1753. The tranſlation of the Dublin Pharmacopœia muſt be an acceptable addition, and the introductory Epitome of Modern Chemiſtry, a knowledge of which is indiſpenſable to thoſe who would underſtand Pharmacy as a ſcience, or practiſe it with advantage as an art; was loudly called for by the many improvements made of late years in this branch of ſcience.

It would take up too much room to give the titles of the ſeveral new tables, which are all uſeful: and Dr. D. appears to have availed himſelf of every thing in the field of modern diſcovery, or in the beſt foreign Pharmacopœias, that was conſiſtent with the plan of the work.

Accum's lectures on chemiſtry.

MR. ACCUM, who has reſigned his gratuitous ſervice as Aſſiſtant Chemical Operator in the Royal Inſtitution, is about to enter on a Courſe of Lectures on Practical Chemiſtry, and its Application to Agriculture, Arts, and Manufactures. They will comprehend diſtinct Series of Lectures on Popular Chemiſtry, Operative Chemiſtry, Mineralogical Chemiſtry, Agricultural Chemiſtry, and Galvaniſm.

Drawn by Blunt.

Mr Geo: Smart's

Apparatus for

Cleansing Chimnies.

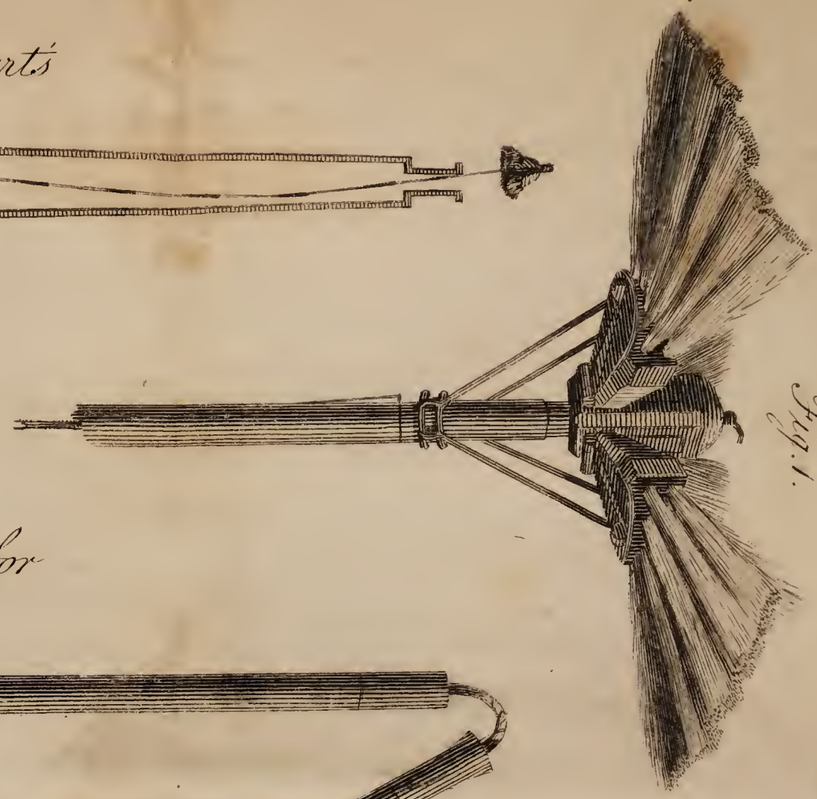


Fig. 1.

Fig. 5.

Fig. 2.

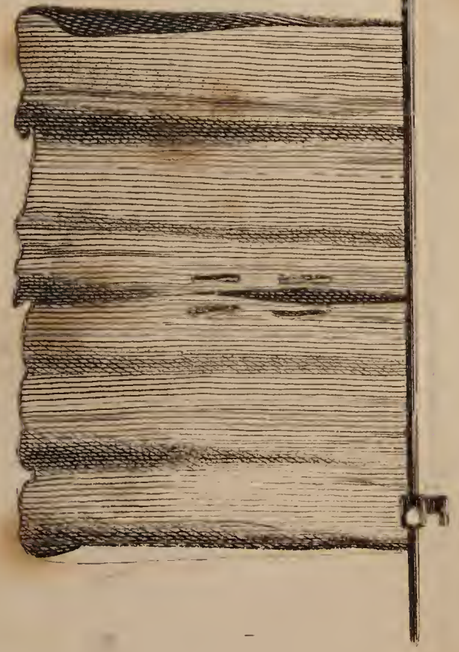
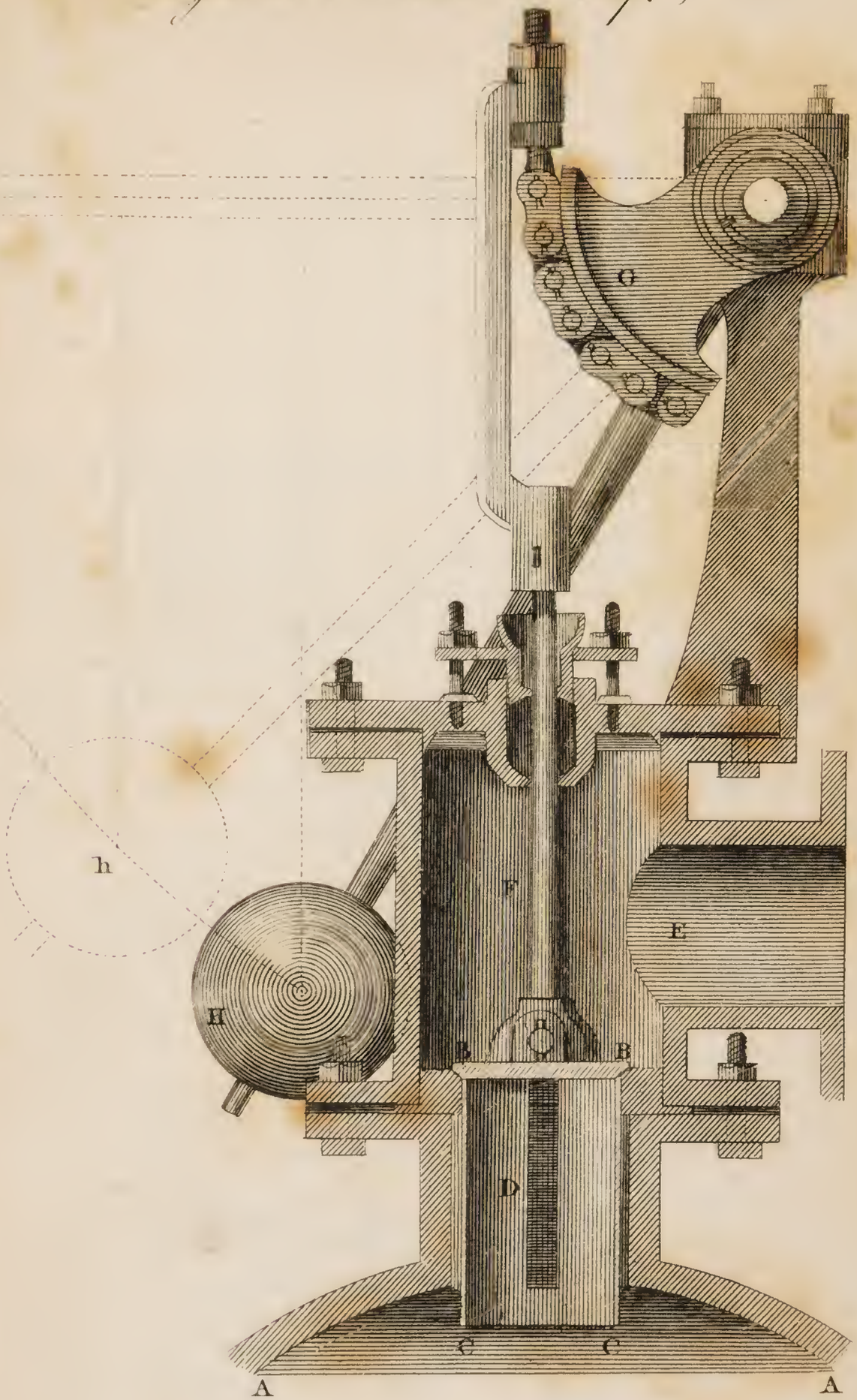


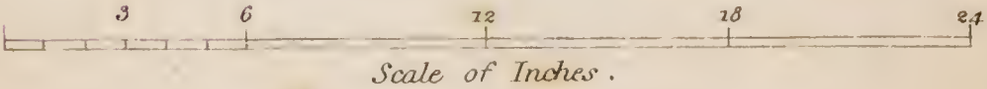
Fig. 3.

Fig. 4.

Engraved by Mudow.

Self acting & regulating Steam Valve: by Mr. Arthur Woolf.





Scale of Inches.

Engraved by Mutlow.

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